A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1801.

ARTICLE I.

Outlines of Obfervations relating to Nitrous Oxide, or Dephlogifticated Nitrous Air. By Mr. DAVY, Director of the Chemical Laboratory, and Lecturer on Chemiftry in the Royal Inftitution. *

I.

SECT. 1. The aëriform fluid, that conftitutes the fubject of the following obfervations, was called by its difcoverer, Dr. Prieftley, dephlogifticated nitrous air. The Dutch chemifts, who, fome time ago, inveftigated certain of its properties, named it gafeous oxide of azote. Lately, in this country, it has been denominated nitrous oxide, which, according to the principles of the French Nomenclature, fignifies nitrogen in the firft degree of oxigenation. This term cannot well be confounded with nitrous gas; † and it bears the neceffary analogy to words denoting other combinations of oxigen and nitrogen.

* Extracted from "Refearches, Chemical and Philofophical, concerning Nitrous Oxide, 1800, Johnfon," with additions, and communicated by the author.

† Nitrous gas, according to the *ftrict* principles of the French Nomenclature, ought to be called *nitric* oxide.

Sect.

Sect. 2. A short and general account of the production and properties of nitrous oxide will not, perhaps, be wholly unacceptable to the chemical world; particularly as the most remarkable agencies of this substance are but little known out of Great Britain; and they are connected with curious physiological enquiries.

II. *On the Production of Nitrous Oxide.*

Sect. 1. As yet nitrous oxide has not been composed immediately from its elements; and there is no reason to believe that it is produced in any of the operations of nature. But it may be procured in a number of artificial processes, during the decomposition of nitrous gas and nitric acid.

Sect. 2. Nitrous gas may be converted into nitrous oxide in two modes. First, by the simple abstraction of a portion of its oxigen by bodies possessing a strong affinity for that principle, such as alkaline sulphites, muriate of tin, and sulphurets. Secondly, by the combination of a body with portions both of its oxigen and nitrogen, such as hidrogen, when in a peculiar state of combination, or in a nascent form.

a. The alkaline sulphites convert nitrous gas into nitrous oxide with much greater rapidity than any other substances.

At temperature 46°, Fahrenheit, 16 cubic inches of nitrous gas were converted in less than an hour into 7,8 cubic inches of nitrous oxide, by 100 grains of pulverized sulphite of pot-ash containing its water of crystallization. During this process, the sulphite became partially converted into sulphate; but no *sensible* increase of temperature was produced. No water was decomposed, nor was any free nitrogen evolved.

Muriate of tin, and the sulphurets,* whether solid or in aqueous solution, act slowly upon nitrous gas, and become oxigenated, whilst they convert it into nitrous oxide. The diminution of the nitrous gas, in these processes, is the same as in the process with sulphite of pot-ash.

b. When nitrous gas and sulphurated hidrogen gas are mingled together, they slowly decompose each other, with great diminution of volume. In this operation, nitrous oxide is formed,† sulphur is deposited, and signs of the production of ammoniac and water are perceived. There is every reason to believe that the hidrogen of the sulphurated hidrogen is the great agent in effecting this decomposition, by combining with portions of both the oxigen and nitrogen of the nitrous gas, to produce water and ammoniac. For no perceptible quantity of acid is formed in the experiment; and the sulphur deposited appears to be in its common state. This opinion indeed is further proved, by the phenomena connected with the decomposition of nitrous gas by nascent hidrogen.

* Dr. Priestley first noticed the conversion of nitrous gas into nitrous oxide by sulphurets. To the Dutch chemists we owe the discovery of the action of muriate of tin on nitrous gas. Journal de Physique, XLIII. part II. page 326.

† Mr. Kirwan first observed the peculiar action of sulphurated hidrogen on nitrous gas. Phil. Transf. Vol. LXXVI. page 133.

When

When nitrous gas is expofed to nafcent hidrogen, whether it be procured by the action of metals upon water, in common cafes, or in Galvanic experiments, ammoniac and nitrous oxide are uniformly produced; and the volume of the nitrous gas diminifhes nearly to the fame extent, as in the procefs with fulphurated hidrogen, *i. e.* to about one-third.

Sect. 3. Nitrous oxide may be procured from nitric acid, when this fubftance is decompofed by certain metallic bodies; and likewife during the mutual decompofition of nitric acid and ammoniac at certain temperatures.

a. During the action of zinc, iron, or tin, upon diluted nitric acid, certain quantities of nitrous oxide are produced, but mingled with nitrous gas and nitrogen. In thefe proceffes, as has been long known, ammoniac is formed; and confequently water is decompofed: a circumftance moft probably connected with the production of the nitrous oxide; for the quantity of the gas is lefs in proportion as the acid is more concentrated.

b. Nitrous oxide, perfectly pure, may be obtained in an eafy manner, and at a cheap rate, from the decompofition of nitrate of ammoniac. This falt, when deprived of as much of its water of cryftallization as poffible, becomes fluid at the temperature of 320°, Fahrenheit, and immediately begins to be converted into water and nitrous oxide.*

One hundred grains of falt, perfectly decompofed at any temperature between 320° and 620°, produces about 57 grains of gas, and 43 of fluid. In experiments on this decompofition the heat muft be carefully applied and regulated; as at temperatures above 800°, nitrate of ammoniac detonates, and becomes changed into nitrous gas, nitric acid, water, and nitrogen.

III. *On the Combinations of Nitrous Oxide.*

Sect. 1. *Pure* water is capable of holding in folution, at the common temperatures of the atmofphere, a quantity of nitrous oxide equal to about half its volume.† Water, impregnated with nitrous oxide, has a fweetifh tafte, which is hardly perceptible, except to delicate organs: and it becomes effervefcent on agitation. It is poffeffed of no action in vegetable colours; and effects no perceptible change in metallic folutions.

Nitrous oxide is expelled unaltered from water at temperatures equal to about 212°. It is likewife incapable of remaining in combination with it, when the preffure of the atmofphere is removed.

Nitrous oxide is expelled from water by muriatic acid, carbonic acid, and fulphurated hidrogen; but it has a ftronger affinity for that fluid than nitrous gas, oxigen, or hidrogen.

* To Mr. Berthollet we owe the difcovery of the products evolved during the flow decompofition of nitrate of ammoniac. See Mem. de Paris, 1785.

† Dr. Prieftley's Experiments and Obfervations, Vol. II. page 81.

Sect.

Sect. 2. Ether, alcohol, and the volatile and fixed oils, diffolve nitrous oxide in larger quantities than water at common temperatures; but without undergoing any material alteration in their properties. The gas is incapable of remaining in combination with them at the temperature of their ebullition, or under the exhaufted receiver.

Sect. 3. Nitrous oxide has as yet been combined with no folid bodies, except the fixed alkalies; and even the affinities of water, alcohol, ether, &c. for this gas appear to be diminifhed when they hold in folution neutral falts and other fubftances. The fulphurets, the fulphites, and the different bodies that act upon nitrous gas, both dry and in folution, effect no alteration in the compofition of nitrous oxide.

Sect. 4. The muria tic, and the fulphureous acid gafes, expand a little when mingled with nitrous oxide; but they are readily feparated from it by water. No other aëriform fluids appear to be poffeffed of the leaft power of action upon this gas at common temperatures.

Sect. 5. None of the alkaline bodies, except pot-afh and foda, have as yet been combined with nitrous oxide; and even thefe fubftances are capable of uniting with it, only when it is in the nafcent ftate.

a. When nitrous oxide in a free ftate is expofed to the dry alkalies, or alkaline earths, at common temperatures, it is neither abforbed nor acted upon. When it is placed in contact with folutions of them in water, a fmall quantity of it is diffolved: this phenomenon, however, appears to depend, in a great meafure, on the water of the folution; for the gas may be expelled from it unaltered at the temperature of its ebullition.

b. To form the combination of pot-afh, or foda, and nitrous oxide, the alkalies, after being mingled in the dry ftate with alkaline fulphites, muft be expofed, together with them, to nitrous gas. The nitrous oxide, as faft as it is formed by the action of the fulphite on the nitrous gas, is abforbed by the free alkali; and thus a permanent union is effected between the two bodies.

c. The compound of nitrous oxide and pot-afh is very foluble in water, but apparently infoluble in alcohol. Its tafte is not very different from that of cauftic pot-afh, perhaps a little more pungent. When acted upon by the acids it gives out nitrous oxide. This gas is likewife difengaged from it in a pure form by the application of a heat above 400°, Fahrenheit. It produces fcintillations when thrown upon red hot charcoal.

d. The properties of the combination of foda and nitrous oxide are not materially different from thofe of the compound of pot-afh and nitrous oxide.

IV. *Of the Action of Heat and Electricity on Nitrous Oxide.*

Sect 1. Nitrous oxide undergoes no change in its compofition at temperatures below thofe of ignition, though expofed to them for ever fo great a length of time. When, however, it is paffed through a porcelain tube, heated red, or when it is acted on for a long while by the electric fpark, a new arrangement of its principles takes place, and it becomes

becomes converted into nitrous acid, and a gas analogous to atmofpheric air, but containing a little lefs oxigen.*

The nitrous acid formed in this procefs is very fmall in quantity, but the unabforbable gas equals about ,8 of the original volume of the nitrous oxide.

V. *On the Decompofition of Nitrous Oxide by Combuftible Bodies.*

Sect. 1. The changes produced in nitrous oxide by heat, are probably intimately connected with the phenomena of its decompofition by the inflammable bodies, which in general require, for their combuftion in this gas, much higher temperatures than thofe at which they burn in atmofpheric air or oxigen. When they are intenfely heated, they act upon it, and become oxigenated, nitrogen being evolved, and much heat and light generated.

During the combuftion of folid and fluid bodies producing flame in nitrous oxide, nitrous acid is formed, moft likely in confequence of a new arrangement of principles produced by the ignition of a part of the gas not in contact with the burning fubftance. Likewife when nitrous oxide in excefs is decompofed by inflammable gafes, nitrous acid, nitrogen, and a little oxigen, are produced, probably from the fame caufe.

a. Pyrophorus, which inflames in nitrous gas and in atmofpheric air, at or even below 40°, Fahrenheit, requires for its combuftion in nitrous oxide a temperature nearly equal to that of ignition.

b. Phofphorus may be fufed and even fublimed in nitrous oxide without effecting any change in it; but when it is introduced into it in a ftate of active inflammation, it burns with intenfely vivid light.

c. Sulphur does not act in nitrous oxide when introduced into it, burning with the feeble blue flame; but if in a ftate of vivid inflammation, it decompofes it with the production of a beautiful rofe-coloured light.

d. Iron and charcoal, when heated white, burn in nitrous oxide with much greater vividnefs than in the atmofphere.

e. The lighted taper, introduced into nitrous oxide, burns at firft as in oxigen; but afterwards with a flame, white in the centre, and blue at the circumference; a phenomenon apparently depending upon the nitrous acid formed in the procefs.

f. Hidrogen, and the compound inflammable gafes, when mingled in certain proportions with nitrous oxide, and acted on by the electric fpark, explode, with the production of much light and heat.†

* Dr. Prieftley firft obferved, that nitrous oxide, after being acted upon by the electric fpark, became immifcible with water; but he did not notice the nitrous acid produced in the procefs: neither did the Dutch chemifts, who repeated his experiments.

† The Society of Chemifts at Amfterdam thought that hidrogen was the only body capable of decompofing nitrous oxide. Journal de Phyfique, tom. XLIII. part II. page 331. From their experiments it appears probable that in many cafes they did not raife the temperatures of other inflammable bodies fufficiently high for this purpofe.

Sect.

Sect. 2. When nitrous acid is formed in processes of combustion in nitrous oxide, so as to remain in the residual gas, the quantity of free nitrogen evolved never equals more than $\frac{5}{6}$ of the volume of the nitrous oxide decomposed, though when this substance is not produced, 10 of nitrous oxide give out about 11 of nitrogen.

Sect. 3. The heat produced by even the slow combustion of bodies in nitrous oxide, is in all cases sufficient to keep up the process for a certain time, after the original heating cause is removed: but in proportion as the volume of nitrogen increases, the inflammation diminishes in vividness, and it uniformly ceases before the whole of the gas is decomposed. Charcoal and iron cease to burn in nitrous oxide when it is mingled with an equal volume of nitrogen; yet the combustion of phosphorus will continue (though feebly) in a gas consisting of 4 nitrogen and 1 nitrous oxide.

VI. *On the Analysis of Nitrous Oxide.*

Sect. 1. *a.* One hundred cubic inches of nitrous oxide weigh at temperature 55°, Fahrenheit, and atmospheric pressure 30. about 50.2 grains.

b. Four cubic inches of this gas, when perfectly decomposed by the combustion of charcoal, are converted into about 2.2 cubic inches of carbonic acid, and 4.2 cubic inches of nitrogen; no nitrous acid being produced in the process.

c. Likewise 2 cubic inches of nitrous oxide require, for their complete decomposition, about 2.2 cubic inches of hidrogen, and evolve during the inflammation, a quantity of nitrogen, nearly equal to it in volume.

b. Comparing these facts with other facts relating to the production of nitrous oxide, we may conclude that 100 parts of this gas are composed of about 37 parts oxigen and 63 parts nitrogen, * existing in a much more condensed state than when in their simple forms.

c. The oxigen appears to be combined with the nitrogen much more intimately in nitrous oxide than in nitrous gas; a phenomenon that may be easily accounted for, when we consider the general law of saturation. †

VII. *On the Action of Nitrous Oxide on Living Beings.*

Sect. 1. Nitrous oxide when taken into the mouth tastes faintly sweetish. Its odour is weak, but agreeable. It is respirable, and produces very peculiar effects, when made to act on the system by means of the lungs.

The sensations it occasions are in general analogous to those connected with intoxication from fermented liquors.

* This estimation is very analogous to that of the Dutch chemists. Journal de Physique, tom. XLIII. part II. page 331.

† The affinity of one body for another is inversely as its saturation with it.

a. Healthy

a. Healthy perſons after breathing it accurately for a minute and a half, or two minutes, are uſually exhilarated, and affected by ſtrong pleaſurable ſenſations, the circulation is quickened, the countenance becomes darker, and a ſenſe of warmth is induced.

b. On unhealthy perſons the effects are leſs pleaſant, and in certain caſes it has induced hyſterical affections.

c. Nitrous oxide after being breathed for more than three minutes, produces violent excitement, which generally ends in momentary loſs of ſenſation.

d. As far as experiments have yet gone, there is no reaſon to believe that any notable debility follows, even its moſt violent agency.

Sect. 2. Warm blooded animals when permanently immerſed in nitrous oxide, live longer in it than in any other gaſes, except ſuch as contain free oxigen; but cold blooded animals are quickly deſtroyed by it.

a. The ſmaller quadrupeds die in pure nitrous oxide in three or four minutes; they abſorb a little of the gas, and their death ſeems to be occaſioned by exceſſive ſtimulation.

b. When examined after death their internal parts are found to have undergone very peculiar changes.

The animal fibre is very inirritable; the blood both in the arteries and veins is dark and purple, and the lungs are marked with large purple ſpots.

Sect. 3. When nitrous oxide is expoſed to fluid venous blood, whether in the veſſels or out of them, it is rapidly abſorbed, apparently no portion of it is decompoſed, and the colour of the blood becomes rather more inclined to purple.

II.

Obſervations on the ſuppoſed Magnetic Property of Nickel, by RICHARD CHEVENIX, *Eſq. F. R. S. and M. R. I. A. Communicated by the Author.*

THE properties of magnetiſm and polarity, firſt obſerved in iron, were, for a long time, the attributes of it alone. But the experiments of later philoſophers had given reaſon to ſuppoſe, that nickel and cobalt participate of both. The celebrated Bergman, in his diſſertation upon the former of theſe two metals, after lamenting the inſufficiency of all his attempts to deprive it of the laſt portions of iron with which it was contaminated, concludes the paragraph with a doubt, whether magnetiſm may not be a property of nickel, as well as of iron. To this the authority of Arvidſon may be added; and hence chymiſts have ſuppoſed, that magnetiſm is not a property of iron excluſively. In Italy, within theſe five years, as I have been informed, compaſſes for marine purpoſes have been made of nickel and of cobalt; thus being preferable to iron, as better reſiſting the action of the air. I know it was the opinion of the French chymiſts, that magnetic needles, made of either

either of thoſe metals perfectly pure, anſwered the purpoſe as well as thoſe made of the uſual ſubſtance. In the ſummer of 1798 I was witneſs to ſome experiments, performed at the *Ecole des mines*, to ſhew that ſeveral pieces of both, which had been reduced to the metallic ſtate, in the laboratory of that eſtabliſhment, and which were magnetic, were totally exempt from iron. The experiments were conducted by Monſ. Vauquelin, and were as follows: ſome magnetic bits of each of thoſe metals were diſſolved in an acid, and iron was, in vain, looked for by the delicate teſts of an infuſion of gall nuts, and a ſolution of pruſſiate of pot-aſh. Hence it was reaſonable to conclude that no iron was preſent, and that nickel and cobalt really are magnetic of themſelves, and independently of alloy.

If I have obtained nickel in its metallic ſtate, and in no way attractable, it was not in performing experiments with this direct view, for ſo fully was I convinced of the accuracy of thoſe I had witneſſed in Paris in 1798, that I entertained no doubt upon the ſubject. I had an acid ſolution of impure oxide of nickel, which I precipitated by ammonia, for the purpoſe of procuring ſome pure nickel which I happened to want. I filtered immediately, and ſuffered the ammoniacal ſolution to ſtand ſome days in a well ſtopped bottle. The liquor remained of the uſual colour, but a blueiſh precipitate, which likewiſe I ſeparated by the filtre, had been formed at the bottom. The alkaline liquor, which had paſſed clear, was evaporated; and the oxide of nickel, which fell as the ammonia was volatilized, was collected, waſhed, dried, and treated with a proper flux, in a Heſſian crucible, and in a forge capable of giving a ſtrong heat. By this proceſs I obtained a reſult which was nickel, ſo unattractable, that preſented as near as a ſteady hand could hold it, to a very delicate magnet, ſupported on a cork floating on water or on mercury, no viſible influence took place upon the magnet. A bit of the nickel, diſſolved in an acid, exhibited, as may be well ſuppoſed, no traces of iron.

Its ſpecific gravity, taken by the Count de Bournon, at a balance ſenſible to the $\frac{1}{200}$ of a grain, and at the temperature of 60, was 7,3806, a ſpecific gravity much inferior to that which Bergman had found, 9,0000: and even to that given by Monſ. de Morveau, 7,8070; hence it would appear that the preſence of iron tends to increaſe its denſity. Its fuſibility I could not aſcertain, having no pyrometer. Its malleability is certainly greater than that of zinc. Its colour, like ſome of the alloys of ſilver and copper, of a very pale red. Its hardneſs conſiderable, and its fracture grainy and uneven. Several of my chymical friends in this country have witneſſed the inefficacy of this nickel in attracting the magnetic needle; and Profeſſor Pictet of Geneva was ſo good as to take ſome of it to the chymiſts in France, to convince them of the error which they are under, with regard to this property in nickel.

It does not appear from the reſults of the experiments to which, as I mentioned, I was a witneſs in 1798, that there was any inaccuracy in the mode of operating; but merely, that magnetiſm is a re-agent more powerful to detect minute portions of iron, when in its metallic ſtate, than is either a ſolution of gall nuts, or of pruſſiate of pot-aſh, to diſcover

4 its

its oxide when held in ſolution by acid menſtrua. Indeed the magnet will, in advantageous circumſtances, be influenced by the ſmalleſt particle of metallic iron adhering to the ſurface of any body, without regard to the actual quantity of the ſubſtance to which it is attached; while the efficacy of chymical teſts is, in ſome meaſure, bounded by the proportion of heterogeneous elements. In one caſe the action is independent and abſolute; in the other, relative and limited. Thus a lump of copper, of whatever ſize, being ſtruck with an iron hammer upon an anvil, ſometimes acquires the power of influencing the magnet; but I believe it would be rather difficult to diſcover, by reagents, the iron that confers on it that power; and which perhaps does not amount to a millionth part of the whole maſs.

As far as I have obſerved, the following cautions alone are neceſſary in thus treating the oxide of nickel by ammonia.

1. All oxides of iron, except the red, are ſoluble, more or leſs, in ammonia; therefore the iron muſt, by the means of nitric acid, be brought to its full ſtate of oxidation, before precipitation and rediſſolution.

2. The ammonia muſt be perfectly cauſtic; for carbonate of ammonia diſſolves red oxide of iron. And,

3. It may be expedient to allow ſome precipitate of nickel to be formed at the bottom, before the ammoniacal liquor is drawn off for evaporation; as, by that time, we may be ſure, that if the two firſt conditions have been obſerved, all the oxide of iron has previouſly fallen to the bottom.

Although I have not directly performed a ſimilar operation upon cobalt, or obtained that metal unattractable, I have little doubt but that ſuch a quality is intirely owing to iron; and that the property of magnetiſm muſt be again reſtricted to its former limits.

III.

On the Mechaniſm of the Eye. By Thomas Young, *M.D. F.R.S.*

(Continued from page 265.)

THESE conſiderations will explain the inconſiſtency of different obſervations on the refractive power of the cryſtalline; and, in particular, how the refraction which I formerly calculated, from meaſuring the focal length of the lens,* is ſo much greater than that which is determined by other means. But, for direct experiments, Dr. Wollaſton's method is exceedingly accurate.

* Phil. Tranſ. for 1793, p. 174.

When

When I look at a minute lucid point, fuch as the image of a candle in a fmall concave fpeculum, it appears as a radiated ftar, as a crofs, or as an unequal line, and never as a perfect point, unlefs I apply a concave lens inclined at a proper angle, to correct the unequal refraction of my eye. If I bring the point very near, it fpreads into a furface nearly circular, and almoft equably illuminated, except fome faint lines, nearly in a radiating direction. For this purpofe, the beft image is a candle, or a fmall fpeculum, viewed through a minute lens at fome little diftance, or feen by reflection in a larger lens. If any preffure has been applied to the eye, fuch as that of the finger keeping it fhut, the fight is often confufed for a fhort time after the removal of the finger, and the image is in this cafe fpotty or curdled. The radiating lines are probably occafioned by fome flight inequalities in the furface of the lens, which is very fuperficially furrowed in the direction of its fibres: the curdled appearance will be explained hereafter. When the point is further removed, the image becomes evidently oval, the vertical diameter being longeft, and the lines a little more diftinct than before, the light being ftrongeft in the neighbourhood of the centre; but immediately at the centre there is a darker fpot, owing to fuch a flight depreffion at the vertex as is often obfervable in examining the lens after death. The fituation of the rays is conftant, though not regular; the moft confpicuous are feven or eight in number; fometimes about twenty fainter ones may be counted. Removing the point a little further, the image becomes a fhort vertical line; the rays that diverged horizontally being perfectly collected, while the vertical rays are ftill feparate. In the next ftage, which is the moft perfect focus, the line fpreads in the middle, and approaches nearly to a fquare, with projecting angles, but is marked with fome darker lines towards the diagonals. The fquare then flattens into a rhombus, and the rhombus into a horizontal line unequally bright. At every greater diftance, the line lengthens, and acquires alfo breadth, by radiations fhooting out from it, but does not become a uniform furface, the central part remaining always confiderably brighteft, in confequence of the fame flattening of the vertex which before made it fainter. Some of thefe figures bear a confiderable analogy to the images derived from the refraction of oblique rays, (Schol. 4. Prop. IV.) and ftill more ftrongly refemble a combination of two of them in oppofite directions; fo as to leave no doubt, but that both furfaces of the lens are oblique to the vifual axis, and co-operate in diftorting the focal point. This may alfo be verified, by obferving the image delineated by a common glafs lens, when inclined to the incident rays. (See Plate XV. Fig. 28—40.)

The vifual axis being fixed in any direction, I can at the fame time fee a luminous object placed laterally at a confiderable diftance from it; but in various directions the angle is very different. Upwards it extends to 50 degrees, inwards to 60, downwards to 70, and outwards to 90 degrees. Thefe internal limits of the field of view nearly correfpond with the external limits formed by the different parts of the face, when the eye is directed forwards and fomewhat downwards, which is its moft natural pofition; although the internal limits are a little more extenfive than the external; and both are well calculated for enabling us to perceive the moft readily, fuch objects as are the moft likely to concern us.

Dr. Wollaston's eye has a larger field of view, both vertically and horizontally, but nearly in the same proportions, except that it extends further upwards. It is well known, that the retina advances further forwards towards the internal angle of the eye, than towards the external angle; but upwards and downwards its extent is nearly equal, and is indeed every way greater than the limits of the field of view, even if allowance is made for the refraction of the cornea only. The sensible portion seems to coincide more nearly with the painted choroid of quadrupeds: but the whole extent of perfect vision is little more than 10 degrees; or, more strictly speaking, the imperfection begins within a degree or two of the visual axis, and at the distance of 5 or 6 degrees becomes nearly stationary, until, at a still greater distance, vision is wholly extinguished. The imperfection is partly owing to the unavoidable aberration of oblique rays, but principally to the insensibility of the retina: for, if the image of the sun itself be received on a part of the retina remote from the axis, the impression will not be sufficiently strong to form a permanent spectrum, although an object of very moderate brightness will produce this effect when directly viewed. It would probably have been inconsistent with the economy of nature, to bestow a larger share of sensibility on the retina. The optic nerve is at present very large; and the delicacy of the organ renders it, even at present, very susceptible of injury from slight irritation, and very liable to inflammatory affections; and, in order to make the sight so perfect as it is, it was necessary to confine that perfection within narrow limits. The motion of the eye has a range of about 55 degrees in every direction; so that the field of perfect vision, in succession, is by this motion extended to 110 degrees.

But the whole of the retina is of such a form as to receive the most perfect image, on every part of its surface, that the state of each refracted pencil will admit; and the varying density of the crystalline renders that state more capable of delineating such a picture, than any other imaginable contrivance could have done. To illustrate this, I have constructed a diagram, representing the successive images of a distant object filling the whole extent of view, as they would be formed by the successive refractions of the different surfaces. Taking the scale of my own eye, I am obliged to substitute, for a series of objects at any indefinitely great distance, a circle of 10 inches radius; and it is most convenient to consider only those rays which pass through the anterior vertex of the lens; since the actual centre of each pencil must be in the ray which passes through the centre of the pupil, and the short distance of the vertex of the lens from this point, will always tend to correct the unequal refraction of oblique rays. The first curve (Plate XV. Fig. 16.) is the image formed by the furthest intersection of rays refracted at the cornea; the second, the image formed by the nearest intersection; the distance between these, shows the degree of confusion in the image; and the third curve, its brightest part. Such must be the form of the image which the cornea tends to delineate in an eye deprived of the crystalline lens; nor can any external remedy properly correct the imperfection of lateral vision. The next three curves show the images formed after the refraction at the anterior surface of the lens, distinguished in the same manner; and the three following, the result of all the successive refractions.

refractions. The tenth curve is a repetition of the ninth, with a slight correction near the axis, at F, where, from the breadth of the pupil, some perpendicular rays must fall. By comparing this with the eleventh, which is the form of the retina, it will appear that nothing more is wanting for their perfect coincidence, than a moderate diminution of density in the lateral parts of the lens. If the law, by which this density varies, were more accurately ascertained, its effect on the image might be estimated by means of the eighth proposition; and probably the image, thus corrected, would approach very nearly to the form of the twelfth curve.

To find the place of the entrance of the optic nerve, I fix two candles at ten inches distance, retire sixteen feet, and direct my eye to a point four feet to the right or left of the middle of the space between them: they are then lost in a confused spot of light; but any inclination of the eye brings one or the other of them into the field of view. In Bernoulli's eye, a greater deviation was required for the direction of the axis;* and the obscured part appeared to be of greater extent. From the experiment here related, the distance of the centre of the optic nerve from the visual axis is found (by Prop. V.) to be 16 hundredths of an inch; and the diameter of the most insensible part of the retina, one-thirtieth of an inch. In order to ascertain the distance of the optic nerve from the point opposite to the pupil, I took the sclerotica of the human eye, divided it into segments, from the centre of the cornea towards the optic nerve, and extended it on a plane. I then measured the longest and shortest distances from the cornea to the perforation made by the nerve, and their difference was exactly one-fifth of an inch. To this we must add a fiftieth, on account of the eccentricity of the pupil in the uvea, which in the eye that I measured was not great, and the distance of the centre of the nerve from the point opposite the pupil will be 11 hundredths. Hence it appears, that the visual axis is five hundredths, or one-twentieth of an inch, further from the optic nerve than the point opposite the pupil. It is possible that this distance may be different in different eyes: in mine, the obliquity of the lens, and the eccentricity of the pupil with respect to it, will tend to throw a direct ray upon it, without much inclination of the whole eye; and it is not improbable, that the eye is also turned slightly outwards, if looking at any object before it, although the inclination is too small to be subjected to measurement.

It must also be observed, that it is very difficult to ascertain the proportions of the eye so exactly as to determine, with certainty, the size of an image on the retina; the situation, curvature, and constitution of the lens, make so material a difference in the result, that there may possibly be an error of almost one-tenth of the whole. In order, therefore, to obtain some confirmation from experiment, I placed two candles at a small distance from each other, turned the eye inwards, and applied the ring of a key so as to produce a spectrum, of which the edge coincided with the inner candle; then, fixing my eye on the outward one, I found that the spectrum advanced over two-sevenths of the distance between them.

* Comm. Petrop. I. p. 314.

Hence,

Hence, the same portion of the retina that subtended an angle of seven parts at the centre of motion of the eye, subtended an angle of five at the supposed intersection of the principal rays; (Plate XIV. Fig. 11.) and the distance of this intersection from the retina was 637 thousandths. This nearly corresponds with the former calculation; nor can the distance of the centre of the optic nerve from the point of most perfect vision be, on any supposition, much less than that which is here assigned. And, in the eyes of quadrupeds, the most strongly painted part of the choroid is further from the nerve than the real axis of the eye.

I have endeavoured to express in four figures, the form of every part of my eye, as nearly as I have been able to ascertain it; the first (Pl. XV. Fig. 17.) is a vertical section; the second (Fig. 18.) a horizontal section; the third and fourth are front views, in different states of the pupil. (Fig. 19 and 20.)

Considering how little inconvenience is experienced from so material an inequality in the refraction of the lens as I have described, we have no reason to expect a very accurate provision for correcting the aberration of the lateral rays. But, as far as can be ascertained by the optometer, the aberration arising from figure is completely corrected; since four or more images of the same line appear to meet exactly in the same point, which they would not do if the lateral rays were materially more refracted than the rays near the axis. The figure of the surfaces is sometimes, and perhaps always, more or less hyperbolical * or elliptical: in the interior laminæ indeed, the solid angle of the margin is somewhat rounded off; but the weaker refractive power of the external parts, must greatly tend to correct the aberration arising from the too great curvature towards the margin of the disc. Had the refractive power been uniform, it might have collected the lateral rays of a direct pencil nearly as well; but it would have been less adapted to oblique pencils of rays; and the eye must also have been encumbered with a mass of much greater density than is now required, even for the central parts: and, if the whole lens had been smaller, it would also have admitted too little light. It is possible too, that Mr. Ramsden's observation, † on the advantage of having no reflecting surface, may be well-founded: but it has not been demonstrated, that less light is lost in passing through a medium of variable density, than in a sudden transition from one part of that medium to another; nor are we yet sufficiently acquainted with the cause of this reflection, to be enabled to reason satisfactorily on the subject. But, neither this gradation, nor any other provision, has the effect of rendering the eye perfectly achromatic. Dr. Jurin had remarked this, long ago, ‡ from observing the colour bordering the image of an object seen indistinctly. Dr. Wollaston pointed out to me on the optometer, the red and blue appearance of the opposite internal angles of the crossing lines; and mentioned, at the same time, a very elegant experiment for proving the dispersive power of the eye. He looks through a prism at a small lucid point, which of course

* Petit Mém. de l'Acad. 1725, p. 20.

† Phil. Trans. for 1795, p. 2.

‡ Smith, e. 96.

becomes

becomes a linear fpectrum. But the eye cannot fo adapt itfelf as to make the whole fpectrum appear a line; for, if the focus be adapted to collect the red rays to a point, the blue will be too much refracted, and expand into a furface; and the reverfe will happen if the eye be adapted to the blue rays; fo that, in either cafe, the line will be feen as a triangular fpace. The obfervation is confirmed, by placing a fmall concave fpeculum in different parts of a prifmatic fpectrum, and afcertaining the utmoft diftances at which the eye can collect the rays of different colours to a focus. By thefe means I find, that the red rays, from a point at 12 inches diftance, are as much refracted as white or yellow light at 11. The difference is equal to the refraction of a lens 132 inches in focus. But the aberration of the red rays in a lens of crown glafs, of equal mean refractive power with the eye, would be equivalent to the effect of a lens 44 inches in focus. If, therefore, we can depend upon this calculation, the difperfive power of the eye collectively, is one-third of the difperfive power of crown glafs, at an equal angle of deviation. I cannot obferve much aberration in the violet rays. This may be, in part, owing to their faintnefs; but yet I think their aberration muft be lefs than that of the red rays. I believe it was Mr. Ramfden's opinion, that fince the feparation of coloured rays is only obferved where there is a fudden change of denfity, fuch a body as the lens, of a denfity gradually varying, would have no effect whatever in feparating the rays of different colours. If this hypothefis fhould appear to be well-founded, we muft attribute the whole difperfion to the aqueous humour; and its difperfive power will be half that of crown glafs, at the fame deviation. But we have an inftance, in the atmofphere, of a very gradual change of denfity; and yet Mr. Gilpin informs me, that the ftars, when near the horizon, appear very evidently coloured. At a more favourable feafon of the year, it would not be difficult to afcertain, by means of the optometer, the difperfive power of the eye, and of its different parts, with greater accuracy than by the experiment here related. Had the difperfive power of the whole eye been equal to that of flint glafs, the diftances of perfect vifion would have varied from 12 inches to 7 for different rays, in the fame ftate of the mean refractive powers.

VII. The faculty of accommodating the eye to various diftances, appears to exift in very different degrees in different individuals. The fhorteft diftance of perfect vifion in my eye, is 26 tenths of an inch for horizontal, and 29 for vertical rays. This power is equivalent to the addition of a lens of 4 inches focus. Dr. Wollafton can fee at 7 inches, and with converging rays; the difference anfwering to 6 inches focal length. Mr. Abernethy has perfect vifion from 3 inches to 30, or a power equal to that of a lens $3\frac{1}{3}$ inches in focus. A young lady of my acquaintance can fee at 2 inches and at 4; the difference being equivalent to 4 inches focus. A middle aged lady at 3 and at 4; the power of accommodation being only equal to the effect of a lens of 12 inches focus. In general, I have reafon to think, that the faculty diminifhes in fome degree, as perfons advance in life; but fome alfo of a middle age appear to poffefs it in a very fmall degree. I fhall take the range of my own eye, as being probably about the medium, and inquire what changes will be neceffary

neceffary in order to produce it; whether we fuppofe the radius of the cornea to be diminifhed, or the diftance of the lens from the retina to be increafed, or thefe two caufes to act conjointly, or the figure of the lens itfelf to undergo an alteration.

1. We have calculated, that when the eye is in a ftate of relaxation, the refraction of the cornea is fuch as to collect rays diverging from a point ten inches diftant, to a focus at the diftance of $13\frac{2}{7}$ tenths. In order that it may bring to the fame focus, rays diverging from a point diftant 29 tenths, we find (by Cor. 5, Prop. IV.) that its radius muft be diminifhed from 31 to 25 hundredths, or very nearly in the ratio of five to four.

2. Suppofing the change from perfect vifion at ten inches to 29 tenths, to be effected by a removal of the retina to a greater diftance from the lens, this will require, (by the fame Corollary,) an elongation of 135 thoufandths, or more than one-feventh of the diameter of the eye. In Mr. Abernethy's eye, an elongation of 17 hundredths, or more than one-fixth, is requifite.

3. If the radius of the cornea be diminifhed one-fixteenth, or to 29 hundredths, the eye muft at the fame time be elongated 97 thoufandths, or about one-ninth of its diameter.

4. Suppofing the cryftalline lens to change its form; if it became a fphere, its diameter would be 28 hundredths, and, its anterior furface retaining its fituation, the eye would have perfect vifion at the diftance of an inch and a half. (Cor. 5 and 8, Prop. IV.) This is more than double the actual change. But it is impoffible to determine precifely how great an alteration of form is neceffary, without afcertaining the nature of the curves into which its furfaces may be changed. If it were always a fpheroid more or lefs oblate, the focal length of each furface would vary inverfely as the fquare of the axis: but, if the furfaces became, from fpherical, portions of hyperbolic conoids, or of oblong fpheroids, or changed from more obtufe to more acute figures of this kind, the focal length would vary more rapidly. Difregarding the elongation of the axis, and fuppofing the curvature of each furface to be changed proportionally, the radius of the anterior muft become about 21, and that of the pofterior 15 hundredths.

VIII. I fhall now proceed to inquire, which of thefe changes takes place in nature; and I fhall begin with a relation of experiments made in order to afcertain the curvature of the cornea in all circumftances.

The method defcribed in Mr. Home's Croonian Lecture for 1795,* appears to be far preferable to the apparatus of the preceding year: † for a difference in the diftance of two images feen in the cornea, would be far greater, and more confpicuous, than a change of its prominency, and far lefs liable to be difturbed by accidental caufes. It is nearly, and perhaps totally impoffible to change the focus of the eye, without fome motion of its axis. The eyes fympathize perfectly with each other; and the change of focus is almoft infeparable from a change of the relative fituation of the optic axes; fo much, that if I direct both my eyes at an object beyond their furtheft focus, I cannot avoid bringing that focus a little

* Phil. Tranf. for 1796, p. 2.

† Phil. Tranf. for 1795, p. 13.

nearer;

nearer: while one axis moves, it is not eafy to keep the other perfectly at reft; and it is not impoffible, that a change in the proportions of fome eyes, may render a flight alteration of the pofition of the axis abfolutely neceffary. Thefe confiderations may partly explain the trifling difference in the place of the cornea that was obferved in 1794. It appears that the experiments of 1795 were made with confiderable accuracy, and no doubt with excellent inftruments; and their failing to afcertain the exiftence of any change, induced Mr. Home and Mr. Ramfden to abandon, in great meafure, the opinion which fuggefted them, and to fuppofe, that a change of the cornea produces only one-third of the effect. Dr. Olbers of Bremen, who in the year 1780 publifhed a moft elaborate differtation on the internal changes of the eye,* which he lately prefented to the Royal Society, had been equally unfuccefsful in his attempts to meafure this change of the cornea, at the fame time that his opinion was in favour of its exiftence.

Room was however ftill left for a repetition of the experiments; and I began with an apparatus nearly refembling that which Mr. Home has defcribed. I had an excellent achromatic microfcope, made by Mr. Ramfden for my friend Mr. John Ellis, of five inches focal length, magnifying about 20 times. To this I adapted a cancellated micrometer, in the focus of the eye not employed in looking through the microfcope: it was a large card, divided by horizontal and vertical lines into fortieths of an inch. When the image in the microfcope was compared with this fcale, care was taken to place the head fo that the relative motion of the images on the micrometer, caufed by the unfteadinefs of the optic axis, fhould always be in the direction of the horizontal lines, and that there could be no error, from this motion, in the dimenfions of the image taken vertically. I placed two candles fo as to exhibit images in a vertical pofition in the eye of Mr. Konig, who had the goodnefs to affift me; and, having brought them into the field of the microfcope, where they occupied 35 of the fmall divifions, I defired him to fix his eye on objects at different diftances in the fame direction; but I could not perceive the leaft variation in the diftance of the images.

Finding a confiderable difficulty in a proper adjuftment of the microfcope, and being able to depend on my naked eye in meafuring diftances, without an error of one 500th of an inch, I determined to make a fimilar experiment without any magnifying power. I conftructed a divided eye-glafs of two portions of a lens, fo fmall, that they paffed between two images reflected from my own eye; and, looking in a glafs, I brought the apparent places of the images to coincide, and then made the change for viewing nearer objects; but the images ftill coincided. Neither could I obferve any change in the images reflected from the other eye, where they could be viewed with greater convenience, as they did not interfere with the eye-glafs. But, not being at that time aware of the perfect fympathy of the eyes, I thought it moft certain to confine my obfervation to the one with which I faw. I

* De Oculi Mutationibus internis. Gotting. 1780. 4°.

muſt remark that, by a little habit, I have acquired a very ready command over the accommodation of my eye, ſo as to be able to view an object with attention, without adjuſting my eye to its diſtance.

I alſo ſtretched two threads, a little inclined to each other, acroſs a ring, and divided them by ſpots of ink into equal ſpaces. I then fixed a ring, applied my eye cloſe behind it, and placed two candles in proper ſituations before me, and a third on one ſide, to illuminate the threads. Then, ſetting a ſmall looking-glaſs, firſt at four inches diſtance, and next at two, I looked at the images reflected in it, and obſerved at what part of the threads they exactly reached acroſs in each caſe; and with the ſame reſult as before.

I next fixed the cancellated micrometer at a proper diſtance, illuminated it ſtrongly, and viewed it through a pin-hole, by which means it became diſtinct in every ſtate of the eye; and, looking with the other eye into a ſmall glaſs, I compared the image with the micrometer in the manner already deſcribed. I then changed the focal diſtance of the eye, ſo that the lucid points appeared to ſpread into ſurfaces, from being too remote for perfect viſion; and I noted on the ſcale, the diſtance of their centres; but that diſtance was invariable.

Laſtly, I drew a diagonal ſcale, with a diamond, on a looking-glaſs, (Plate XIV. Fig. 12.) and brought the images into contact with the lines of the ſcale. Then, ſince the image of the eye occupies on the ſurface of a glaſs half its real dimenſions, at whatever diſtance it is viewed, its true ſize is always double the meaſure thus obtained. I illuminated the glaſs ſtrongly, and made a perforation in a narrow ſlip of black card, which I held between the images; and was thus enabled to compare them with the ſcale, although their apparent diſtance was double that of the ſcale. I viewed them in all ſtates of the eye; but I could perceive no variation in the interval between them.

The ſufficiency of theſe methods may be thus demonſtrated. Make a preſſure along the edge of the upper eyelid with any ſmall cylinder, for inſtance a pencil, and the optometer will ſhow that the focus of horizontal rays is a little elongated, while that of vertical rays is ſhortened; an effect which can only be owing to a change of curvature in the cornea. Not only the apparatus here deſcribed, but even the eye unaſſiſted, will be capable of diſcovering a conſiderable change in the images reflected from the cornea, although the change be much ſmaller than that which is requiſite for the accommodation of the eye to different diſtances. On the whole, I cannot heſitate to conclude, that if the radius of the cornea were diminiſhed but one-twentieth, the change would be very readily perceptible by ſome of the experiments related; and the whole alteration of the eye requires one-fifth.

But a much more accurate and deciſive experiment remains. I take out of a ſmall botanical microſcope, a double convex lens, of eight-tenths radius and focal diſtance, fixed in a ſocket one-fifth of an inch in depth; ſecuring its edges with wax, I drop into it a little water, nearly cold, till it is three-fourths full, and then apply it to my eye, ſo that the cornea enters half way into the ſocket, and is every where in contact with the water. (Plate XIV. Fig. 13.) My eye immediately becomes preſbyopic, and the refractive power of

of the lens, which is reduced by the water to a focal length of about 16 tenths, (Cor. 5, Prop. IV.) is not fufficient to fupply the place of the cornea, rendered inefficacious by the intervention of the water; but the addition of another lens, of five inches and a half focus, reftores my eye to its natural ftate, and fomewhat more. I then apply the optometer, and I find the fame inequality in the horizontal and vertical refractions as without the water; and I have, in both directions, a power of accommodation equivalent to a focal length of four inches, as before. As firft fight indeed, the accommodation appears to be fomewhat lefs, and only able to bring the eye from the ftate fitted for parallel rays to a focus at five inches diftance; and this made me once imagine, that the cornea might have fome flight effect in the natural ftate; but, confidering that the artificial cornea was about a tenth of an inch before the place of the natural cornea, I calculated the effect of this difference, and found it exactly fufficient to account for the diminution of the range of vifion. I cannot afcertain the diftance of the glafs lens from the cornea to the hundredth of an inch; but the error cannot be much greater, and it may be on either fide.

After this, it is almoft neceffary to apologize for having ftated the former experiments; but, in fo delicate a fubject, we cannot have too great a variety of concurring evidence.

IX. Having fatisfied myfelf that the cornea is not concerned in the accommodation of the eye, my next object was to inquire if any alteration in the length of its axis could be difcovered; for this appeared to be the only poffible alternative: and, confidering that fuch a change muft amount to one-feventh of the diameter of the eye, I flattered myfelf with the expectation of fubmitting it to meafurement. Now, if the axis of the eye were elongated one-feventh, its tranfverfe diameter muft be diminifhed one-fourteenth, and the femi-diameter would be fhortened a thirtieth of an inch.

I therefore placed two candles fo that when the eye was turned inwards, and directed towards its own image in a glafs, the light reflected from one of the candles by the fclerotica appeared upon its external margin, fo as to define it diftinctly by a bright line; and the image of the other candle was feen in the centre of the cornea. I then applied the double eye-glafs, and the fcale of the looking-glafs, in the manner already defcribed; but neither of them indicated any diminution of the diftance, when the focal length of the eye was changed.

Another teft, and a much more delicate one, was the application of the ring of a key at the external angle, when the eye was turned as much inwards as poffible, and confined at the fame time by a ftrong oval iron ring, preffed againft it at the internal angle. The key was forced in as far as the fenfibility of the integuments would admit, and was wedged, by a moderate preffure, between the eye and the bone. In this fituation, the phantom caufed by the preffure extended within the field of perfect vifion, and was very accurately defined; nor did it, as I formerly imagined, by any means prevent a diftinct perception of the objects actually feen in that direction; and a ftraight line coming within the field of this oval phantom, appeared fomewhat inflected towards its centre; (Plate XIV. Fig. 14.) a dif-

a diftortion eafily underftood by confidering the effect of the preffure on the form of the retina. Suppofing now, the diftance between the key and the iron ring to have been, as it really was, invariable, the elongation of the eye muft have been either totally or very nearly prevented; and, inftead of an increafe of the length of the eye's axis, the oval fpot caufed by the preffure would have fpread over a fpace at leaft ten times as large as the moft fenfible part of the retina. But no fuch circumftance took place: the power of accommodation was as extenfive as ever; and there was no perceptible change, either in the fize or in the figure of the oval fpot.

Again, fince the rays which pafs through the centre of the pupil, or rather the anterior vertex of the lens, may, as already obferved, be confidered as delineating the image; and, fince the divergence of thefe rays with refpect to each other, is but little affected by the refraction of the lens, they may ftill be faid to diverge from the centre of the pupil; and the image of a given object on the retina muft be very confiderably enlarged, by the removal of the retina to a greater diftance from the pupil and lens. (Cor. Prop. V*.) To afcertain the real magnitude of the image with accuracy, is not fo eafy as it at firft fight appears; but, befides the experiment laft related, which might be employed as an argument to this purpofe, there are two other methods of eftimating it. The firft is too hazardous to be of much ufe; but, with proper precautions, it may be attempted. I fix my eye on a brafs circle placed in the rays of the fun, and, after fome time, remove it to the cancellated micrometer; then, changing the focus of my eye, while the micrometer remains at a given diftance, I endeavour to difcover whether there is any difference in the apparent magnitude of the fpectrum on the fcale; but I can difcern none. I have not infifted on the attempt; efpecially as I have not been able to make the fpectrum diftinct enough without inconvenience; and no light is fufficiently ftrong to caufe a permanent impreffion on any part of the retina remote from the vifual axis. I therefore had recourfe to another experiment. I placed two candles fo as exactly to anfwer to the extent of the termination of the optic nerve, and, marking accurately the point to which my eye was directed, I made the utmoft change in its focal length; expecting that, if there were any elongation of the axis, the external candle would appear to recede outwards upon the vifible fpace. (Plate XIV. Fig. 15.) But this did not happen; the apparent place of the obfcure part was precifely the fame as before. I will not undertake to fay, that I could have obferved a very minute difference either way: but I am perfuaded, that I fhould have difcovered an alteration of lefs than a tenth part of the whole.

It may be inquired if no change in the magnitude of the image is to be expected on any other fuppofition; and it will appear to be poffible, that the changes of curvature may be

* This Corollary fhould ftand thus. "If a confufed image be received on any given plane, it will be neceffary, in order to determine its magnitude, to advert to the aperture admitting the rays. If the aperture be fuppofed to be infinitely fmall, it may be confidered as a radiant point, in order to find the direction of the emergent rays."

 fo

fo adapted, that the magnitude of the confufed image may remain perfectly conftant. Indeed, to calculate from the dimenfions which we have hitherto ufed, it would be expected that the image fhould be diminifhed about one-fortieth, by the utmoft increafe of the convexity of the lens. But the whole depends on the fituation of the refracting furfaces, and the refpective increafe of their curvature, which, on account of the variable denfity of the lens, can fcarcely be eftimated with fufficient accuracy. Had the pupil been placed before the cornea, the magnitude of the image muft, on any fuppofition, have been very variable: at prefent, this inconvenience is avoided by the fituation of the pupil; fo that we have here an additional inftance of the perfection of this admirable organ.

From the experiments related, it appears to be highly improbable that any material change in the length of the axis actually takes place; and it is almoft impoffible to conceive by what power fuch a change could be effected. The ftraight mufcles, with the adipofe fubftance lying under them, would certainly, when acting independently of the focket, tend to flatten the eye: for, fince their contraction would neceffarily leffen the circumference or fuperficies of the mafs that they contain, and round off all its prominences, their attachment about the nerve and the anterior part of the eye muft therefore be brought nearer together. (Plate XV. Fig. 21, 22.) Dr. Olbers compares the mufcles and the eye to a cone, of which the fides are protruded, and would by contraction be brought into a ftraight line. But this would require a force to preferve the cornea as a fixed point, at a given diftance from the origin of the mufcles; a force which certainly does not exift. In the natural fituation of the vifual axis, the orbit being conical, the eye might be fomewhat lengthened, although irregularly, by being forced further into it; but, when turned towards either fide, the fame action would rather fhorten its axis; nor is there any thing about the human eye that could fupply its place. In quadrupeds, the oblique mufcles are wider than in man; and in many fituations might affift in the effect. Indeed a portion of the orbicular mufcle of the globe is attached fo near to the nerve, that it might alfo cooperate in the action: and I have no reafon to doubt the accuracy of Dr. Olbers, who ftates, that he effected a confiderable elongation, by tying threads to the mufcles, in the eyes of hogs and of calves; yet he does not fay in what pofition the axis was fixed; and the flaccidity of the eye after death might render fuch a change very eafy as would be impoffible in a living eye. Dr. Olbers alfo mentions an obfervation of Profeffor Wrifberg, on the eye of a man whom he believed to be deftitute of the power of accommodation in his life-time, and whom he found, after death, to have wanted one or more of the mufcles: but this want of accommodation was not at all accurately afcertained. I meafured, in the human eye, the diftance of the attachment of the inferior oblique mufcle from the infertion of the nerve: it was one-fifth of an inch; and from the centre of vifion not a tenth of an inch; fo that, although the oblique mufcles do in fome pofitions nearly form a part of a great circle round the eye, their action would be more fitted to flatten than to elongate it. We have therefore reafon to agree with Winflow, in attributing to them the

the office of helping to support the eye on that side where the bones are most deficient: they seem also well calculated to prevent its being drawn too much backwards by the action of the straight muscles. And, even if there were no difficulty in supposing the muscles to elongate the eye in every position, yet at least some small difference would be expected in the extent of the change, when the eye is in different situations, at an interval of more than a right angle from each other; but the optometer shews that there is none.

Dr. Hosack alleges that he was able, by making a pressure on the eye, to accommodate it to a nearer object: * it does not appear that he made use of very accurate means of ascertaining the fact; but, if such an effect took place, the cause must have been an inflection of the cornea.

It is unnecessary to dwell on the opinion which supposes a joint operation, of changes in the curvature of the cornea and in the length of the axis. This opinion had derived very great respectability, from the most ingenious and elegant manner in which Dr. Olbers had treated it, and from being the last result of the investigation of Mr. Home and Mr. Ramsden. But either of the series of experiments which have been related, appears to be sufficient to confute it.

X. It now remains to inquire into the pretensions of the crystalline lens to the power of altering the focal length of the eye. The grand objection to the efficacy of a change of figure in the lens, was derived from the experiments in which those who have been deprived of it have appeared to possess the faculty of accommodation.

My friend Mr. Ware, convinced as he was of the neatness and accuracy of the experiments related in the Croonian Lecture for 1795, yet could not still help imagining, from the obvious advantage all his patients found, after the extraction of the lens, in using two kinds of spectacles, that there must, in such cases, be a deficiency in that faculty. This circumstance, combined with a consideration of the directions very judiciously given by Dr. Porterfield, for ascertaining the point in question, first made me wish to repeat the experiments upon various individuals, and with the instrument which I have above described as an improvement of Dr. Porterfield's optometer: and I must here acknowledge my great obligation to Mr. Ware, for the readiness and liberality with which he introduced me to such of his numerous patients as he thought most likely to furnish a satisfactory determination. It is unnecessary to enumerate every particular experiment; but the universal result is, contrary to the expectation with which I entered on the inquiry, that in an eye deprived of the crystalline lens, the actual focal distance is totally unchangeable. This will appear from a selection of the most decisive observations.

1. Mr. R. can read at four inches and at six only, with the same glass. He saw the double lines meeting at three inches, and always at the same point; but the cornea was somewhat irregularly prominent, and his vision not very distinct; nor had I, at the time I saw him, a convenient apparatus.

* Phil. Transf. for 1794, p. 212.

I after-

I afterwards provided a ſmall optometer, with a lens of leſs than two inches focus, adding a ſeries of letters, not in alphabetical order, and projected into ſuch a form as to be moſt legible at a ſmall inclination. The exceſs of the magnifying power had the advantage of making the lines more divergent, and their croſſing more conſpicuous; and the letters ſerved for more readily naming the diſtance of the interſection, and, at the ſame time, for judging of the extent of the power of diſtinguiſhing objects too near or too remote for perfect viſion. (Plate XV. Fig. 23.)

2. Mr. J. had not an eye very proper for the experiment; but he appeared to diſtinguiſh the letters at $2\frac{1}{2}$ inches, and at leſs than an inch. This at firſt perſuaded me, that he muſt have a power of changing the focal diſtance: but I afterwards recollected that he had withdrawn his eye conſiderably, to look at the nearer letters, and had alſo partly cloſed his eyelids, no doubt contracting at the ſame time the aperture of the pupil; an action which, even in a perfect eye, always accompanies the change of focus. The ſlider was not applied.

3. Miſs H. a young lady of about twenty, had a very narrow pupil, and I had not an opportunity of trying the ſmall optometer: but, when ſhe once ſaw an object double through the ſlits, no exertion could make it appear ſingle at the ſame diſtance. She uſed for diſtant objects a glaſs of $4\frac{1}{2}$ inches focus; with this ſhe could read as far off as 12 inches, and as near as five: for nearer objects ſhe added another of equal focus, and could then read at 7 inches, and at $2\frac{1}{2}$.

4. Hanſon, a carpenter, aged 63, had a cataract extracted a few years ſince from one eye: the pupil was clear and large, and he ſaw well to work with a lens of $2\frac{3}{8}$ inches focus; and could read at 8 and at 15 inches, but moſt conveniently at 11. With the ſame glaſs, the lines of the optometer appeared always to meet at 11 inches; but he could not perceive that they croſſed, the line being too ſtrong, and the interſection too diſtant. The experiment was afterwards repeated with the ſmall optometer: he read the letters from 2 to 3 inches; but the interſection was always at $2\frac{1}{2}$ inches. He now fully underſtood the circumſtances that were to be noticed, and ſaw the croſſing with perfect diſtinctneſs: at one time, he ſaid it was a tenth of an inch nearer; but I obſerved that he had removed his eye two or three tenths from the glaſs, a circumſtance which accounted for this ſmall difference.

5. Notwithſtanding Hanſon's age, I conſider him as a very fair ſubject for the experiment. But a ſtill more unexceptionable eye was that of Mrs. Maberly. She is about 30, and had the cryſtalline of both eyes extracted a few years ſince, but ſees beſt with her right. She walks without glaſſes; and, with the aſſiſtance of a lens of about four inches focus, can read and work with eaſe. She could diſtinguiſh the letters of the ſmall optometer from an inch to $2\frac{1}{2}$ inches; but the interſection was invariably at the ſame point, about 19 tenths of an inch diſtant. A portion of the capſule is ſtretched acroſs the pupil, and cauſes her to ſee remote objects double, when without her glaſſes; nor can ſhe, by any

any exertion, bring the two images nearer together, although the exertion makes them more distinct, no doubt by contracting the pupil. The experiment with the optometer was conducted, in the presence of Mr. Ware, with patience and perseverance; nor was any opinion given to make her report partial.

Considering the difficulty of finding an eye perfectly suitable for the experiments, these proofs may be deemed tolerably satisfactory. But, since one positive argument will counterbalance many negative ones, provided it be equally grounded on fact, it becomes necessary to inquire into the competency of the evidence employed to ascertain the power of accommodation attributed, in the Croonian Lecture for 1794, to the eye of Benjamin Clerk. And it appears, that the distinction long since very properly made by Dr. Jurin, between distinct vision and perfect vision, will readily explain away the whole of that evidence.

It is obvious that vision may be made distinct to any given extent, by means of an aperture sufficiently small, provided at the same time, that a sufficient quantity of light be left, while the refractive powers of the eye remain unchanged. And it is remarkable, that in those experiments, when the comparison with the perfect eye was made, the aperture of the imperfect eye only was very considerably reduced. Benjamin Clerk, with an aperture of $\frac{3}{40}$ of an inch, could read with the same glass at $1\frac{7}{8}$ inch, and at 7 inches.* With an equal aperture, I can read at $1\frac{1}{2}$ inch and at 30 inches: and I can retain the state of perfect relaxation, and read with the same aperture at $2\frac{1}{4}$ inches; and this is as great a difference as was observed in Benjamin's Clerk's eye. It is also a fact of no small importance, that Sir Henry Englefield was much astonished, as well as the other observers, at the accuracy with which the man's eye was adjusted to the same distance, in the repeated trials that were made with it.† This circumstance alone makes it highly probable, that its perfect vision was confined within very narrow limits.

(To be continued.)

IV.

Description of a Support applicable to Balances of all Dimensions, and proper to render the Operations performed with these Instruments more expeditious and convenient without affecting their Precision.‡ *By* R. PRONY, *of the National Institute of France.*

SOME experiments which I was engaged in during the course of last winter obliged me to contrive a support, to which the beam of any balance whatever might be applied without distinction, whether provided with its frame of support or not, and which, without de-

* Phil. Transf. for 1795, p. 9. † Phil. Transf. for 1795, p. 8.

‡ Annales de Chimie, XXXVI. 50.

prived

priving it in any respect of its precision, should afford a convenient means of raising and lowering it at pleasure. It is well known how troublesome it is to weigh by balances held in the hand, and this trouble is frequently by no means the smallest inconvenience attending that method.

Several artists have invented supports, convenient in their use, and ingenious in their contrivance; but each of these is adapted only to a single balance, and increases the price in such a manner as to put it out of the convenience of most philosophers and artists. I hope I shall therefore give them pleasure by publishing, in consequence of the invitation of several celebrated chemists, the description of a support, which, to the advantage of being universal, adds that of cheapness and easy execution, whether in wood or in metal.

It was my intention to have added to this description some views respecting the theory and construction of balances, from which I apprehend that artists acquainted with the elements of geography and calculation might derive some advantage in the improvement of machines for weighing; but as this part of my work is more extended than I at first thought, I shall publish it separately.

A triangular foot of brass, A *a*, *a*, *a*,, (Plate XVI. Fig. 1 & 2) has its three extremities, *a*, *a*″ *a*‴, solidly screwed to a table or horizontal plane. In the part A of this foot is screwed a cylindrical stem A B, of any convenient length. It may be proper to have two of different lengths, in order to exchange them when very large balances are used. Those which I have used are of half a metre and a metre in length.

A vertical pulley is placed at the top of the rod A B, so that the same vertical plane passes through the axis of the beam and the horizontal axis of the pulley. The support C D of this pulley has at its lower part a cylindrical bore, into which the upper extremity of the rod A B enters with an easy friction; and a thumb-screw E serves by its pressure to keep the pulley in its fixed position.

Another pulley P is fixed below the rod A B, so that the tangent line to the pullies P *p* is parallel to the axis of the rod A B.

A string K *t p* H G P F, at the end of which is suspended, beyond the edge of the table K, a small weight, *k* passes through a hole *t*, made in the foot *a*″, thence over the pullies *p* and P, and is fastened at F to the upper part of a piece *m m*` *n q*, which is forked, and to which are suspended, as I shall proceed to explain, the balance, the weights, and the objects to be weighed. F *m* is a button, which, being screwed to the top of the fork, receives the string through a hole, in which it remains by virtue of a knot made in its extremity.

The tail, or lower part of this fork, is prismatic in its part *m*′ *n*; and this prismatic part enters into a notch *f f* made in the extremity N of the horizontal piece N O, in which it can freely slide upwards and downwards through a limited space, its ascent being stopped by the enlargement of the fork below, and above by the enlargement occasioned by the separation of the two branches of the fork.

4 The

The piece N O is perforated, and may be ſlided up and down the ſtem A B, and alſo moved horizontally to the right and left. When it has the proper height, it is fixed, by means of the preſſing ſcrew V, that is to ſay, 1ſt. when its height is ſuch that the ſtop *m′* reſts on the edge of the notch *f f*, or when N O, being at its loweſt limit, the diſhes of the balance are in contact with the table or horizontal plane, ſo that it may be raiſed afterwards through the whole height *f n*; and, 2dly, that the ſtring F F ſhall be in the ſame vertical plane with H G.

The notch in N muſt be ſo placed that the axis of the priſmatic part of the tail of the fork and the ſtring F F″ ſhall be always in the ſame vertical line parallel to the axis of the ſtem A B.

Theſe arrangements being underſtood, let us imagine the two branches *n q* of the fork to be drilled through with holes of different ſizes, to receive the horizontal pins *g g* of different diameters, and we ſhall poſſeſs an apparatus quite adequate to weighing in the air with balances which have their beams ſupported between the uſual frame and rings.

Whatever, in fact, may be the ſize of the balance intended to be uſed, the extremity of its clip, or frame, is to be introduced between the fork *n q*, and a pin of ſuch a ſize as to enter very freely into the hole at the top of the clip muſt be paſſed through; after which the piece O N is to be placed, ſo that the conditions before mentioned with regard to this piece ſhall be obtained; and in this poſition it is to be fixed with the thumb-ſcrew V. This operation being made, the diſhes of the balance are to be loaded, which, being in contact with the table or horizontal plane, cannot move. The ſmall ball *k* being then taken in the hand, the ſtring muſt be drawn ſo that the balance ſhall riſe very ſlowly. If the ſcales be not in equilibrium, the beam muſt be ſuffered to deſcend, and the proceſs of weighing be completed by altering the load as uſual.

A counterpoiſe Q, ſuſpended on the ſtring F G, forms an equilibrium to the weight of the balance. By means of this precaution, the reſult of all the efforts which ſupport the pulley P paſſes in all caſes through the axis of the rod A B, which, conſequently, will have no diſpoſition to bend or incline.

But if it be propoſed to uſe an hydroſtatical balance, a ſmall table O′ N, Fig. 3, is to be adapted to the rod A B, which, by means of a cylindrical perforation in O′, can ſlide up and down the rod, and be fixed at any required height by the thumb-ſcrew V′. Another piece, or little table, K′ K, is placed upon V N, ſo that the holes T correſpond to the centre of the diſhes, below which are placed the hooks intended to hold the body ſuſpended in water, and K′ K′ is fixed upon V′ N′ by means of the ſcrew V′.

This adjuſtment being prepared, the piece N O is to be placed on the table K′ K′, ſo that, 1ſt. the whole height of the balance ſhall be comprehended between this piece and the ſmall table, and the diſhes ſhall be contact with the ſame, their centers correſponding with the openings made in T; 2d. that K K′ ſhall be ſufficiently raiſed to admit of the veſſels W W of water being placed beneath, ſo as conveniently to immerſe the body intended to be weighed in one of theſe veſſels.

These

Thefe bodies are ufually fufpended by a very thin metallic wire; but, by placing two veffels as I have done, and fufpending to each of the bafons a wire of metal of the fame diameter, one to fupport the body, and the other to be fimply immerfed in the water, the magnitude of the diameter will have no influence on the accuracy of the experiment. In fact, if we fuppofe the machine to be fo adjufted that at the commencement of the operation the difhes fhall be in equilibrio (which is eafily effected by varying the height of the water in the veffels), thefe two wires will continue to form an equilibrium, when the beam F F″ being raifed ftill preferves its horizontal pofition. Whence it follows that if to one of the wires there be fufpended an immerfed body, and in the oppofite bafon there be placed out of the water a weight capable of maintaining the equilibrium, that equilibrium will continue, whatever may be the elevation or depreffion of the whole balance, provided it continues horizontal. For the lengths of the wires raifed out of the water being equal, the difference between the fpecific gravities of the water and the metal will have the fame influence on the two extremities of the beam. It is evident that the fame advantage could not be had by employing only one wire; namely, that which fupports the fufpended body, and that the equilibrium correfponding with a certain elevation, and the horizontal pofition of the beam, will not agree with other elevations and the fame horizontal pofition.

It muft be obferved that my procefs not only compenfates for the excefs of fpecific gravity of the wires beyond that of the water, but alfo for the adhefion of the fluid and that portion which hangs round the wires, and is raifed with them.

The whole of the preceding account is applicable to balances fufpended in the ufual clip, or forked piece, terminating in rings for fupporting the fulcrum. But in order to render my fupport abfolutely univerfal, it muft be adapted to a beam alone, the knife edges of which fhall find a proper place to act upon: and in this I have fucceeded, by contriving a forked piece, which hangs from the fork *n q*, and is capable of receiving the knife edges of any beam whatever. In the drawing this piece is properly placed in the machine, and its conftruction is as follows:

A piece S *s*′ (fig. 2.) is pierced at *s*′ by a tapped hole, into which the fcrew *d d* enters to half its length. Another hole made in S, perpendicular to the former, receives the pin *g g*, (fig. 1.) which fufpends the whole lower apparatus, and anfwers to the hole in the upper part of the fork of common balances.

Two other vertical pieces *r r*, Fig. 4, have at their upper part holes not tapped, in which the fcrew *d d* may freely move. Thefe upper ends are to be placed at an arbitrary diftance, where they are retained, and fixed by four fcrew-nuts *u*, each piece being thus confined between two fcrews. A cylindrical rod *h h* paffes through the lower extremities of thefe pieces *r r*, and is fixed by preffing fcrews; fo that the upper and lower extremities of thofe pieces are invariably at the fame diftance.

Each of thefe pieces *r r* has on its face, which is perpendicular to the direction of *d d* and *h h*, a long opening *f f*, and a circular perforation X, (fig. 1. and 5.) the lower part of which is armed with a fmall face of polifhed fteel, upon which the edge of the fulcrum refts;

rests; in the upper part of the openings *f f*, a rule *e′ e′* is introduced by tight friction, which, together with the pieces *d d* and *h h*, give to the frame such a degree of solidity, that there is not theleast shake or play in any of its parts. That part of its opening which is not occupied by *e′-e′* must be rather longer than the longest index of the beams intended to be used.

The use of the frame or fork of support, here described, is extremely simple. The beam is to be placed between the two branches *r r*, which are to be separated till the knife edges are opposite the circular openings X, when they are again to be brought together, so as to include that fulcrum, leaving a little play between the supports, in order that the vibration may be perfectly free. The parallelism of the pieces *r r*, and the perfect solidity of their connection, is then to be effected by means of the screw-nuts, the small cylindric rod *h h*, and the rule *e e*. When these dispositions are made, the whole apparatus is to be suspended by the pin *g g*, in the fork *s*, and the balance is fit for use. The equilibrium, or horizontal position of the beam, is known by the position of the index *y y* in the middle of the opening *e e*; or, to render this estimate more exact, I have suspended to the rule *e e* a plumb-line *e i*, by which the slightest deviations from the perpendicular may be seen.

Such is the description and use of my support, which in practice has perfectly answered my expectation. The instrument I possess is entirely of brass, made by Citizen Dumotier, an artist well known for the construction of philosophical instruments. If the fork, and some of the nicer parts, be made of metal, the rest may be advantageously formed of wood.

V.

On the Combinations of the Metals with Sulphur. By the CITIZEN VAUQUELIN.

THE combinations of the metals with sulphur may be divided into three orders, namely,

1. The metals and sulphur. These are combinations, which properly speaking, ought to be called metallic sulphurets, or sulphurated metals; nature and art afford many specimens of these.

2. Metallic oxides and sulphur. These ought to be called sulphurated metallic oxides, or oxided metallic sulphurets. They are also very common in the native state.

3. Metallic oxides with sulphur and hidrogen. These triple combinations are named hidro-sulphurated metallic oxides.

If the predominating substance in these different combinations be taken as the basis of the nomenclature, it would be necessary to reverse the order of the common denominations; for in the simple sulphurets, the metal always predominates as to its mass; the same is also the case in the sulphurated oxides; and in the hidro-sulphurets the predominance is first in the metals, then in the sulphur, and lastly, in the hidrogen.

So that inftead of metallic fulphurets, oxided metallic fulphurets, and metallic hidro-fulphurets; we ought to fay fulphurated metals, fulphurated metallic oxides, and fulphuro-hidrogenated metallic oxides.

But ufage has prevailed in chemiftry, whether from facility of pronunciation, or the greater fmoothnefs of the terms. However this is of no great importance, and if the elements of bodies be clearly underftood from the terms, it will be fufficient.

Concerning the action of the acids upon the metallic fulphurets.

SULPHURIC ACID.

Whenever the fum of the affinities of oxigen for the metallic fubftance, and of the oxide thus formed for the fulphuric acid, is greater than that of the metal for fulphur and of oxigen for the fulphureous acid, there will conftantly be a decompofitian of the metallic fulphuret, and the refult will be a metallic fulphate and fulphureous acid. The whole of thefe conditions are not always neceffary: but each concurs in the accomplifhment of the new combinations which are formed, and accelerates the operation. There are neverthelefs cafes, in which for want of one of thefe conditions the bodies do not act at all upon each other.

The fulphuric acid for example, decompofes the natural fulphurated oxide of iron or martial pyrites very well; but the muriatic acid cannot decompofe it, becaufe the fulphur in this cafe is not attracted by any body, whereas in the former it is attracted by the fulphuric acid, which it converts into fulphureous acid; for though I have found by many facts, that the iron in martial pyrites is oxided nearly to the point in which it is eafily foluble in acids, it cannot however be decompofed without forming a great quantity of fulphuric acid.

This opinion is confirmed by the conditions neceffary for completing the operation. In fact, the complete decompofition of pyrites cannot be effected without heat, and a great quantity of concentrated fulphuric acid. For it is evident here, that acid is not only required for the faturation of the oxide of iron, but alfo for that of the fulphur. I could likewife urge in fupport of the fame opinion, the greater affinity of the muriatic acid for the oxide of iron, which neverthelefs it is incapable of feparating from the fulphur under thefe circumftances. It may feem ftrange that the nitric acid, of which the affinity to iron is lefs ftrong, fhould neverthelefs decompofe the martial pyrites with fo much facility; but we ought not to deceive ourfelves by fuppofing, that it is by a ftronger affinity that this acid takes the oxide of iron from fulphur; for it is on the contrary, by affording a new quantity of oxigen to the iron that the affinity of the metal for the fulphur becomes fingularly diminifhed. In reality, fulphur does not unite with iron oxided to the maximum, and when by certain peculiar proceffes thefe two fubftances are combined, the effect always happens from a feparation of part of the oxigen.

Though the muriatic acid cannot decompofe the fulphurated oxide of iron, it decompofes on the contrary the fulphuret of iron, in which the metal is not oxigenated, with the greateft facility. But in this cafe three forces act at once—namely, that of the iron on

4 the

the oxigen of the water, that of the ſulphur on the hidrogen of the ſame ſubſtance, and laſtly, that of the metallic oxide formed, for the muriatic acid. In this circumſtance ſulphurated hidrogen gas is conſtantly diſengaged.

The ſulphuric acid which does not decompoſe the ſulphurated oxide of iron, unleſs it be concentrated and boiling, decompoſes the ſulphuret of iron without heat, and the more readily when it is diluted with a certain quantity of water. The reaſon of this evidently requires no explanation.

Though a great quantity of water oppoſes the action of the acids on the metallic ſulphurets in general, nevertheleſs it is leſs noxious with reſpect to that of iron than with regard to the others, becauſe the ſulphurated hidrogen does not decompoſe the ſolutions of iron in the acids.

Concerning the Sulphuret of Lead.

In the natural ſulphuret of lead or galena, lead exiſts in the metallic ſtate; for which reaſon, when this mineral ſubſtance is brought into contact with the muriatic acid, ſulphurated hidrogen is formed even in the cold. It appears even when the ſulphur is united to the oxide of lead; this metal loſes its oxigen in order to unite with the ſulphur, for we obtain a matter perfectly ſimilar to natural galena. The manner in which the reduction of lead is here effected may be eaſily underſtood. A portion of ſulphur combines with the oxigen and forms ſulphuric acid, which the heat volatilizes, and another portion unites with the metallic lead. The reduction of lead and the formation of galena take place nearly in the ſame manner, when a ſolution of this metal in an acid is mixed with a ſolution of ſulphurated hidrogen, or of any hidro-ſulphuret whatever. There is nevertheleſs this difference, namely, that in this caſe the hidrogen takes the oxigen from the lead, and forms water at the ſame time that the ſulphur and the metal, which meet in a ſtate of extreme diviſion, combine without heat in the midſt of the fluid. There is alſo another difference, that the galena formed by this proceſs contains rather more ſulphur than the natural compound, as is proved by the ſmall quantity of ſulphur which ſeparates from this ſubſtance at a very gentle heat, and then leaves it abſolutely in the ſame ſtate as common galena.

From theſe facts it follows that the muriatic acid, and probably all other acids act on the oxide of lead by their maſs and their concentration; that when theſe two forces, or even one of them is deſtroyed, or ſimply weakened, the ſulphurated hidrogen deprives them of the lead; but whatever may be the quantity and concentration of the acid united with the lead or other metals, the decompoſition of the ſalt will take place if an hidroſulphuret be employed in ſufficient quantity.

It is not ſingular that the affinities of the acids for the metallic oxides ſhould be weakened by water, nor that their action on theſe ſubſtances ſhould be more energetic by virtue of their maſſes. Antimony, mercury, biſmuth, and lead itſelf, afford ſtriking examples of this.

It

It is known, in fact, that when a large quantity of water is added to their concentrated solutions, very considerable precipitates are formed, which have been mistakenly supposed to be pure metallic oxides, but which are really salts, containing a portion of acid strongly adherent, that cannot be completely separated but by means of an alkali.

So that we may consider the combinations formed by these metals, and probably many others also with the acids, as neutral salts dissolved by a super-abundance of acid, which is taken from them by the water, and consequently the salts fall down truly neutral and insoluble.

From the manner in which the muriatic acid acts on the metallic sulphurets, it follows, that the more the metals which compose them absorb of oxigen to unite with this acid, the less there remains of sulphur after the solution, and *vice versa*; but if these metals absorb little oxigen, if they contain much sulphur, and if also their affinity for oxigen be inconsiderable, the totality of the metal will never be separated from the sulphur by the muriatic acid, because in proportion as this combustible body becomes more abundant, it opposes by its mass the solution of the metal, and hence it is that certain metallic sulphurets which are not attacked by the muriatic acid, yield to that force, after a portion of their sulphur has been separated by distillation.

The mode of action of the nitric acid on the metallic sulphurets is very different, and though the sulphur is not in this case attracted by any substance, nevertheless it is completely separated from the metal, and falls down perfectly pure if the operation have been properly performed; but though the combination of the nitric acid with the metal is not assisted by hidrogen, as happens with regard to the muriatic acid, yet this first acid yields oxigen to the metal more easily than water; and it is by virtue of this affinity of oxigen for the metal and the oxide which is formed for the undecomposed acid, that the solution is effected. So that in all the instances when metallic sulphurets are decomposed by the nitric acid, there is constantly a formation of nitrous gas, and never of sulphurated hidrogen.

It appears in general, that sulphur has a stronger affinity with the metals than with their oxides, and that this affinity diminishes in proportion to the quantity of oxigen they absorb. There are even some which do not unite with sulphur but in the complete metallic state. Thus for example, by triturating the brown oxide of lead with sulphur, a portion of the sulphur takes fire, and the other portion combines with the lead thus reduced to the metallic state.

It appears also that zinc does not unite with sulphur when it is completely metallic, or at least that nature has not yet presented instances of this, nor has art succeeded in combining these two substances. That which is known in mineralogy by the name of Blende, or sulphurated zinc, has been mistakenly regarded by some mineralogists as a combination of zinc with sulphur. It is on the contrary a sulphurated oxide of zinc, which very frequently contains hidrogen; for the blendes are almost always soluble in acids, and produce sulphu-

rated

rated hidrogen gas. A transparent blende which acts in this manner with the acids, must therefore be considered as the true hidro-sulphurated oxide of zinc, or sulphuro-hidrogenated oxide. There may exist oxides of zinc simply sulphurated, and there would then be two species of ores very different, one soluble in the sulphuric and muriatic acids without effervescence, and the other on the contrary affording a greater or less quantity of sulphurated hidrogen gas.

Metallic mercury does not unite very intimately with sulphur. Mineral ethiops appear rather to be a mixture than a true combination, but when this oxided metal is combined with sulphur, as for example in cinnabar, it adheres so strongly that no substance can directly decompose it, and the separation can never be effected but by altering one or other of these principles. It is either necessary to burn the sulphur, or dis-oxide the mercury. Cinnabar owes its fine red colour to the great quantity of oxigen which the mercury contains.

VI.

Notice respecting the extensive Logarithmic and Trigonometrical Tables, calculated under the Direction of CITIZEN PRONY, *Member of the National Institute, &c. and of the Report concerning the same. By Citizens La Grange, Laplace, and Delambre.*

WHEN in the year 2 of the Republic, C. Prony was charged to calculate the new tables of sines, tangents, &c. and their logarithms as required for the decimal division of the quadrant, he was engaged *not only to compose tables which should leave nothing to be desired with regard to accuracy, but to make them the most extended and most striking monument of calculation which had ever been executed, or even imagined.*

Cit. Prony applied to the execution of this grand enterprize suggested by Citizens Carnot, Prieur de la Côte d'Or, Brunet de Montpellier, which required to be finished in a short time, the principle of the division of labour by means of which the perfection of workmanship, with the saving of expence and time, are obtained in the arts. This division, which renders the most unskilful hands profitable in manual works, permits the useful employ of men of very limited powers in operations, which on the whole are delicate, but abound with simple and minute details. It is much to be wished, that this expedient might continue to be used for the perfection and extent of great scientific works, of which the plan cannot be understood but by men of a superior order, but of which the execution prodigiously exceeds their physical power. Such for example, are the numerical developement of formulæ, which give the perturbation arising from the mutual attraction of different bodies of the planetary system, and the reduction of the astronomical observations proper to determine the elements of the orbits of these bodies.

Citizen

Citizen Prony divided his assistants into three sections.

The first was composed of five or six mathematicians of very great merit, who were employed on the analytical part of the work, and in general on the application of the method of differences to the formation of tables, the calculation of various fundamental numbers, &c. Citizen Legendre, who for some time belonged to this section, gave very elegant formulæ for determining the successive differences of the sines *a priori*.

The second section consisted of calculators to whom the analysis was familiar. They directly calculated the differences of the several orders necessary to form by successive additions, according to the method of interpolation, the values of the trigonometric lines departing from a given first value through the whole interval, wherein the differences of the most elevated order might, without sensible error, be considered as constant.

The filling of these intervals was reduced to simple addition performed by the third section, the papers of which were afterwards transmitted for examination by the second.

The calculators of each section formed two divisions, each of which performed the whole work without communication with the other. The comparison of the results thus obtained afforded the verification.

By these means two manuscript copies of the whole of the table was formed, composed of seventeen volumes large folio, and containing,

1. An introduction, consisting of an exposition of the analytical formulæ, the use of the trigonometrical table, and a great number of particular and auxiliary tables.

2. The natural sines for each for 10,000th part of the quadrant, calculated to twenty-five places of decimals, with seven or eight columns of differences, to be published with twenty-two decimals, and five columns of differences.

3. The logarithms of the sines for each 100,000th of the quadrant, calculated to fourteen decimals with five columns of differences.

4. The logarithms of the ratios of the sines to the arcs for the first five thousand 100,000th part of the quadrant, calculated to fourteen decimals, with three columns of differences.

5. The logarithms of the tangents corresponding with the logarithm of the sines.

6. The logarithms of the ratios of the tangents to the arcs calculated like those of the fourth article.

7. Logarithms of numbers from 1 to 100,000, calculated to nineteen places of decimals.

8. The logarithms of 100,000 to 200,000, calculated to twenty-four decimals, in order to be published to twelve decimals, and three columns of differences.

This explanation shews how far superior this work is to the *Opus Palatinum de Triangulis*, began by Rheticus, the disciple of Copernicus, and compleated in 1596 by Otho, the disciple of Rheticus, or to the Thesaurus Mathematicus, published by Pitiscus in 1613, and the last tables of Vlacq. This was the judgment of the Commissaries, which the Class of the Physical and Mathematical Societies of the National Institute nominated to give an account of

of this work. It is much to be wished, that the government would resume the impression of this immense work, which was suspended at the fall of the assignats.

To the notice, of which we have given an account, Cit. Prony has added a bibliographic anecdote respecting the *Opus Palatinum*, very interesting to those who seek rare books. They will learn with pleasure, that there are a few copies of this book, in which 86 pages have been reprinted, containing the co-tangents and co-secants of the seven first degrees, of which the last figures were uncertain in the first copies, and which Pitiscus could not correct, but by carrying the sines of these degrees to twenty-two decimals, which had been calculated only to fifteen.

This note is followed by the report of Citizens Lagrange, Laplace, and de Lambre, respecting these tables, in which we find various interesting details on the construction of trigonometrical tables in general, and the methods employed by one of the Commissaries to verify those which were submitted to them.

Bulletin de Science, No. 52, An 9.

VII.

Observations relative to the Means of increasing the Quantities of Heat obtained in the Combustion of Fuel. By Count Rumford *.

It is a fact which has been long known, that clays, and several other incombustible substances, when mixed with sea coal, in certain proportions, cause the coal to give out more heat in its combustion, than it can be made to produce when it is burned pure or unmixed; but the cause of this increase of heat does not appear to have been yet investigated with that attention which so extraordinary and important a circumstance seems to demand.

Daily experience teaches us that all bodies,—those which are incombustible,—as well as those which are combustible, and actually burning, throw off in all directions heat, or rather calorific (heat-making) rays, which generate heat wherever they are stopped, or absorbed; but common observation was hardly sufficient to shew any perceptible difference between the quantities of calorific rays thrown off by different bodies, when heated to the same temperature, or exposed in the same fire; although the quantities so thrown off, might be, and probably are, very different.

It has lately been ascertained, that when the sides and back of an open chimney fireplace, in which coals are burned, are composed of fire-bricks, and heated red-hot, they throw off into the room incomparably more heat than all the coals that could possibly be put into the grate, even supposing them to burn with the greatest possible degree of brightness.

* Journal of the Royal Institution, p. 28.

nefs. Hence it appears that a red-hot burning coal does not fend off near fo many calorific rays as a piece of redhot brick or ftone, of the fame form and dimenfions; and this interefting difcovery will enable us to make very important improvements in the conftruction of our fire-places, and alfo in the management of our fires.

The fuel inftead of being employed to heat the room *directly*, or by the direct rays from the fire, fhould be fo difpofed, or placed, as *to heat the back and fides of the grate;* which muft always be conftructed of fire-brick, or fire-ftone, and *never of iron, or of any other metal.* Few coals, therefore, when properly placed, make a much better fire than a larger quantity; and fhallow grates, when they are conftructed of proper materials, throw more heat into a room, and with a much lefs confumption of fuel, than deep grates; for a large mafs of coals in the grate arrefts the rays which proceed from the back and fides of the grate, and prevents their coming into the room; or, as fires are generally managed, it prevents the back and fides of the grate from ever being fufficiently heated to affift much in heating the room, even thongh they be conftructed of good materials, and large quantities of coals be confumed in them.

It is poffible, however, by a fimple contrivance, to make a good and an economical fire in almoft any grate, though it would always be advifable to conftruct fire-places on good principles, or to improve them by judicious alterations, rather than to depend on the ufe of additional inventions for correcting their defects.

To make a good fire in a bad grate, the bottom of the grate muft be firft covered with a fingle layer of balls, made of good fire-bricks, or artificial fire-ftone, well burnt, each ball being perfectly globular, and about 2½ or 2¾ inches in diameter. On this layer of balls the fire is to be kindled, and, in filling the grate, more balls are to be added with the coals that are laid on; care muft, however, be taken in this operation to mix the coals and the balls well together, otherwife, if a number of the balls fhould get together in a heap, they will cool, not being kept red hot by the combuftion of the furrounding fuel, and the fire will appear dull in that part; but if no more than a due proportion of the balls are ufed, and if they are properly mixed with the coals, they will all, except it be thofe perhaps at the bottom of the grate, become red-hot, and the fire will not only be very beautiful, but it will fend off a vaft quantity of radiant heat into the room; and will continue to give out heat for a great length of time. It is the opinion of feveral perfons who have for a confiderable time practifed this method of making their fires, that more than one third of the fuel ufually confumed may be faved by this fimple contrivance. It is very probable that, with careful and judicious management, the faving would amount to one half, or fifty per cent.

As thefe balls, made in moulds, and burnt in a kiln, would coft very little, and as a fet of them would laft a long time, probably feveral years, the faving of expence in heating rooms by chimney fires with bad grates, in this way, is obvious; but ftill, it fhould be remembered that a faving quite as great may be made by altering the grate, and making it a good fire-place.

In

In ufing thefe balls, care muft be taken to prevent their accumulating at the bottom of the grate: as the coals go on to confume, the balls mixed with them will naturally fettle down towards the bottom of the grate, and the tongs muft be ufed occafionally to lift them up; and, as the fire grows low, it will be proper to remove a part of them, and not to replace them in the grate till more coals are introduced: a little experience will fhew how a fire made in this manner can be managed to the greateft advantage, and with the leaft trouble.

Balls made of pieces of any kind of well burnt hard brick, though not equally durable with fire-brick, will anfwer very well, provided they be made perfectly round; but if they are not quite globular their flat fides will get together, and by obftructing the free paffage of the air amongft them, and amongft the coals, will prevent the fire from burning clear and bright.

The beft compofition for making thefe balls, when they are formed in moulds, and afterwards dried and burnt in a kiln, is pounded crucibles mixed up with moiftened Sturbridge clay; but good balls may be made with any very hard burnt common bricks, reduced to a coarfe powder, and mixed with Sturbridge clay, or even with common clay. The balls fhould always be made fo large as not to pafs through between the front bars of a grate.

Thefe balls have one advantage, which is peculiar to them, and which might perhaps recommend the ufe of them to the curious, even in fire-places conftructed on the beft principles; they caufe the cinders to be confumed almoft intirely; and even the very afhes may be burnt, or made to difappear, if care be taken to throw them repeatedly upon the fire when it burns with an intenfe heat. It is not difficult to account for this effect in a fatisfactory manner, and in accounting for it we fhall explain a circumftance on which it is probable that the great increafe of the heat of an open fire, where thefe balls are ufed, may, in fome meafure, depend. The fmall particles of coal, and of cinder, which, in a common fire, fall through the bottom of the grate, and efcape combuftion, when thefe balls are ufed can hardly fail to fall and lodge on fome of them; and, as they are intenfely hot, thefe fmall bodies which alight upon them in their fall, are foon heated red hot, and difpofed to take fire and burn; and, as frefh air from below the grate is continually making its way upwards amongft the balls, every circumftance is highly favourable to the rapid and complete combuftion of thefe fmall inflammable bodies. But if thefe fmall pieces of coal and cinder fhould, in their fall, happen to alight upon the metallic bars which form the bottom of the grate; as thefe bars are conductors of heat, and, on account of that circumftance, as well as of their fituation, *below* the fire, never can be made very hot, any fmall particle of fuel that happens to come into contact with them, not only cannot take fire, but would ceafe to burn, fhould it arrive in a ftate of actual combuftion.

Thefe facts are very important, and well deferving of the attention of thofe who may derive advantage from the improvement of fire-places, and the economy of fuel.

There

There are ſome circumſtances which ſtrongly indicate that an admixture of incombuſtible bodies with fuel, and eſpecially with coal, cauſe an increaſe of the heat, even when the fuel is burnt in a cloſed fire-place. No fire-place can well be contrived more completely cloſed than thoſe of the iron ſtoves in common uſe in the Netherlands; but in theſe ſtoves, which are heated by coal fires, a large proportion of wet-clay is always coarſely mixed with the coals before they are introduced into the fire-place. If this practice had not been found to be uſeful it would certainly never have obtained generally; nor would it have been continued, as it has been, for more than two hundred years.

The combination of different ſubſtances, combuſtible, and incombuſtible, to form, artificially, various kinds of cheap and pleaſant fuel, particularly adapted for the different proceſſes in which the fuel is employed, is a ſubject well worthy of the attention of enterprizing and ingenious men. How much excellent fuel, for inſtance, might be made with proper additions, and proper management, of the mountains of refuſe coal duſt that lie uſeleſs at the mouths of coal pits; and how much would it contribute to cleanlineſs and elegance if the uſe of improved coke, or of hard and light fire balls, could be generally introduced in our houſes and kitchens, inſtead of crude, black, powdery, dirty, ſea coal. Of the great economy that would reſult from ſuch a change there cannot be the ſmalleſt doubt.

It is a melancholy truth, but, at the ſame time, a moſt undiſputable fact, that, while the induſtry and ingenuity of millions are employed, with unceaſing activity, in inventing, improving, and varying thoſe ſuperfluities which wealth and luxury introduce into ſociety, no attention whatever is paid to the improvement of thoſe common neceſſaries of life on which the ſubſiſtence of all, and the comforts and enjoyments of the great majority of mankind, abſolutely depend.

Much will be done for the benefit of ſociety, if means can be deviſed to call the attention of the active and benevolent to this long neglected, but moſt intereſting, ſubject.

The Royal Inſtitution ſeems to be well calculated to facilitate and expedite the accompliſhment of this important object. Indeed it is more than probable that this, preciſely, is the object which was principally had in view in the foundation and arrangement of that eſtabliſhment.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Diſeaſes of horned Cattle and Sheep.

W. P. Whyte, Eſq. of Worceſter, has drawn the outline of a plan for aſcertaining and defining the different diſeaſes of horned cattle and ſheep, their cauſes, and means of prevention or cure. He intends to publiſh a proſpectus ſpeedily; but in the mean time his outline, which is of ſome length, appears in that uſeful work "the Annals of Agriculture" The preſent propoſals are as follow:

1. That a tour ſhall be made at different times through all the principal grazing diſtricts in that part of Great Britain called England, ſimilar to thoſe of Arthur Young, Eſq. or Mr. Marſhall, the writer of the valuable volumes on the Rural Economy of the different Counties, for the purpoſe of aſcertaining and defining the different diſeaſes incident to horned cattle and ſheep, the various cauſes whence they ariſe, and the means of preventing and remedying them.

2. That every particular diſeaſe (ſo far at leaſt as ſuch tour ſhall afford opportunity) ſhall undergo a ſcientific and full inveſtigation, with a view to the different ends propoſed.

3. That minutes of every material occurrence and tranſaction relative to the objects in view ſhall be regularly taken and preſerved, with a view to future publication; in particular, that theſe ſhall record the nature of particular diſeaſes, the diſtricts where they are moſt prevalent, the real, apparent, or probable cauſes of them, the provincial opinions on the ſubject, the ſymptoms of ſuch diſeaſes, the means which have been hitherto employed with intent to remedy them, the ſucceſs or otherwiſe of ſuch means, the morbid anatomical appearances after death, the certain or probable means of prevention, &c.

4. That the reſult of the whole ſhall be publiſhed in octavo volumes, either in diſtinct parts once in every ſix or twelve months, or, arranged in ſyſtematical form, at the concluſion of the tour, as may be thought adviſable by the Secretary to the Board and the Author, upon ſuch terms as ſhall furniſh them to the ſubſcribers at a moderate expence.

5. That the Board ſhall afford their ſanction to the undertaking by a letter of recommendation from the Preſident; which the Touriſt ſhall be at liberty to carry with him, and ſhow occaſionally.

6. That the expence of the undertaking and the remuneration to the Touriſt ſhall be borne and defrayed by a public ſubſcription and the money to ariſe from publication of the minutes.

7. That ſubſcriptions ſhall be received (probably) by Arthur Young, Eſq. F. R. S. Secretary to the Board.

8. That the Royal Family and Nobility of this kingdom, Foreign Potentates and Nobility, and Public Bodies and Societies eſtabliſhed by royal charter, be invited to ſubſcribe at pleaſure.

9. That the Gentry, Land Owners, Breeders, Graziers, and all others ſhall become ſubſcribers upon the payment of   to the Receiver.

10. That a ſufficient number of copies of the minutes ſhall be handſomely printed on a ſuperfine royal quarto paper, and hot preſſed, and delivered *gratis* to all ſubſcribers of any ſum, being five guineas more than the ordinary ſubſcription.

11. That ſo ſoon as   pounds are ſubſcribed, the Touriſt will proceed in the undertaking.

On

On the New Alcali called Pneum of Dr. Hahneman.

Meſſrs. Klaproth, Karſtein, and Hermſtadt having publiſhed in the Literary Gazette of Jenu, that the reſult of their examination of Mr. Hahneman's new alcali was, that it is merely the acid of borax ſuperſaturated with ſoda, that is to common borax. The Doctor publiſhed his anſwer addreſſed to thoſe chemiſts, as follows:

"I may be deceived, but I am not capable of deceiving. I was at work upon crude borax; pot-aſh dropped into a ſolution not yet diſpoſed to cryſtallize, afforded an abundant precipitate in the form of powder. But as authors aſſure us that borax becomes uncryſtallizable by the addition of alcali, it is not ſurprizing that I ſhould have conſidered this precipitate as a new and particular ſubſtance. The re-agents alſo ſhewed phenomena different from thoſe preſented by common borax. But in order to clear it as I thought from all adherent borax, I ſaturated it exactly with acetous acid, and mixed the concentrated ley with equal parts of ſpirit of wine. After the ſeparation of what remained inſoluble, I ſuppoſed myſelf to be in poſſeſſion of the alcaline part, combined with the acetous acid diſſolved in the ſpirit of wine, and I ſeparated this acid by adding one of the three alcalies. The ſalt thus precipitated and waſhed, appeared to me to poſſeſs a ſufficient number of properties different from thoſe of common borax, to be conſidered as a peculiar alcali. The general notion of chemiſts of an unſaturated exceſs of alcali in borax led me into error; for how could this exceſs exiſt in my borax of ſoda, ſince the mineral acids added to my borax to dryneſs eſcaped by heat? How could the exceſs of ſoda exiſt in my borax in a cauſtic ſtate? This exceſs either is not ſoda, or it is not a real exceſs, it is adopted by chemiſts without foundation. This laſt is in fact the caſe. The acid of borax neutralizes the ſoda in ſuch a manner, and extends its neutralizing power in ſuch a manner on the part which has hitherto appeared to us to be in exceſs, that the carbonic acid and the mineral acids ſcarcely enter into any intimate combination with this exceſs."

Journal de Phyſique, Meſſidor An. 9.

Facts and Diſcoveries reſpecting Galvaniſm.

In a letter from Berlin inſerted in the Magazine Encyclopedique I. 1801. pag. 110, dated April laſt, ſeveral experiments are related by Helvige, a Swediſh major, Bourguet, profeſſor of chemiſtry, Hermann, profeſſor of natural philoſophy, and Dr. Grapengieſſer. Theſe philoſophers repeated the experiments made by Volta, together with thoſe which we added in this country. Their pile conſiſted of gold, and zinc with cloth moiſtened with ſalt water. Other metals were uſed, namely, ſilver and copper, inſtead of the gold, without affording any determinate ground for preference, as it appeared to theſe operators. They found that the galvanic ſpark is obtained with much greater facility, when one of the two conductors terminates in very fine pointed iron wire, and the other in a knob or button. This diſcovery preſented itſelf when the gold leaf of the electrometer of Bennet

was

was made part of the circuit; and since making that observation they constantly armed the extremities of theit conductors with gold leaf; by which means they obtained with ease a strong spark, and used the same method with the greatest success in setting fire to phosphorus, sublimed sulphur, fulminating gas, and gunpowder, the latter of which is not easily fired by the electric fluid. It was remarked, that in those experiments the portion of gold leaf which covered the extremity of the conductors, was itself fused and reduced into a globule more or less perfect, according to the force of the spark.

They repeated my experiment with the condenser, and found agreeably to what I had observed, that the silver is in the negative state, and the zinc positive. By the intervention of the condenser they charged a jar, but could not obtain the slightest galvanic commotion. But by means of two condensers (by the process of doubling I suppose) they repeated upon the Electrophore the experiment of producing the figures of Lichtenberg, with the same varieties and forms which would have been produced by the electric fluid.

Dr. Grapengeisser has applied the galvanic energy in the course of medical practice, and observes, that the similitude of this action with that of the electric fluid cannot be mistaken: but these philosophers think, that a great dissimilitude, or variation in these two powers, was indicated by an experiment they made of insulating the whole apparatus, together with the tube in which the process of decomposing water was going forward. In this situation they electrified the whole apparatus by the common machine, and obtained very considerable sparks, without producing any change in the phænomena between the wires in the tube. Hence they think that those powers may be imagined to be independent of each other. Like many philosophers in this country, as well as elsewhere, they are disposed to think that the galvanic power depends upon the transition of oxigen; but it may be observed, that there are many unequivocal facts, which seem to prove that the common electricity also is never excited, but by the condensation or developement of the same principle.

Galvanic Flash.

Mr. Cruickshank has favoured me with the remark, that a mistake appears in the use of the words zinc-face and silver face, at page 240 of our Journal for September, where an account is given of the luminous signs of galvanism corresponding with the two electricities. I here relate the object in his own words: *the brush or flash always appears at the "extremity of the wire, which if immersed in water would disengage hidrogen, or what has been "generally called the silver wire, whereas the zinc, or oxigen wire, exhibit only a faint luminous "point."* And as he perfectly agrees in the remarks made at the page last quoted, respecting the plus and minus electricities, he is disposed to suspect, that there may be some mistake in the original experiments, which were made to ascertain the state as to plus and minus of the extremities of the pile. I will repeat those experiments, in order that the facts may clear up this matter, and shew whether there be any striking difference between the galvanic and electric lights.

Effect

Effect of Galvanifm in a Cafe of Paralyfis of the Mufcles of the Left Side of the Face. By CITIZEN HALLE *.

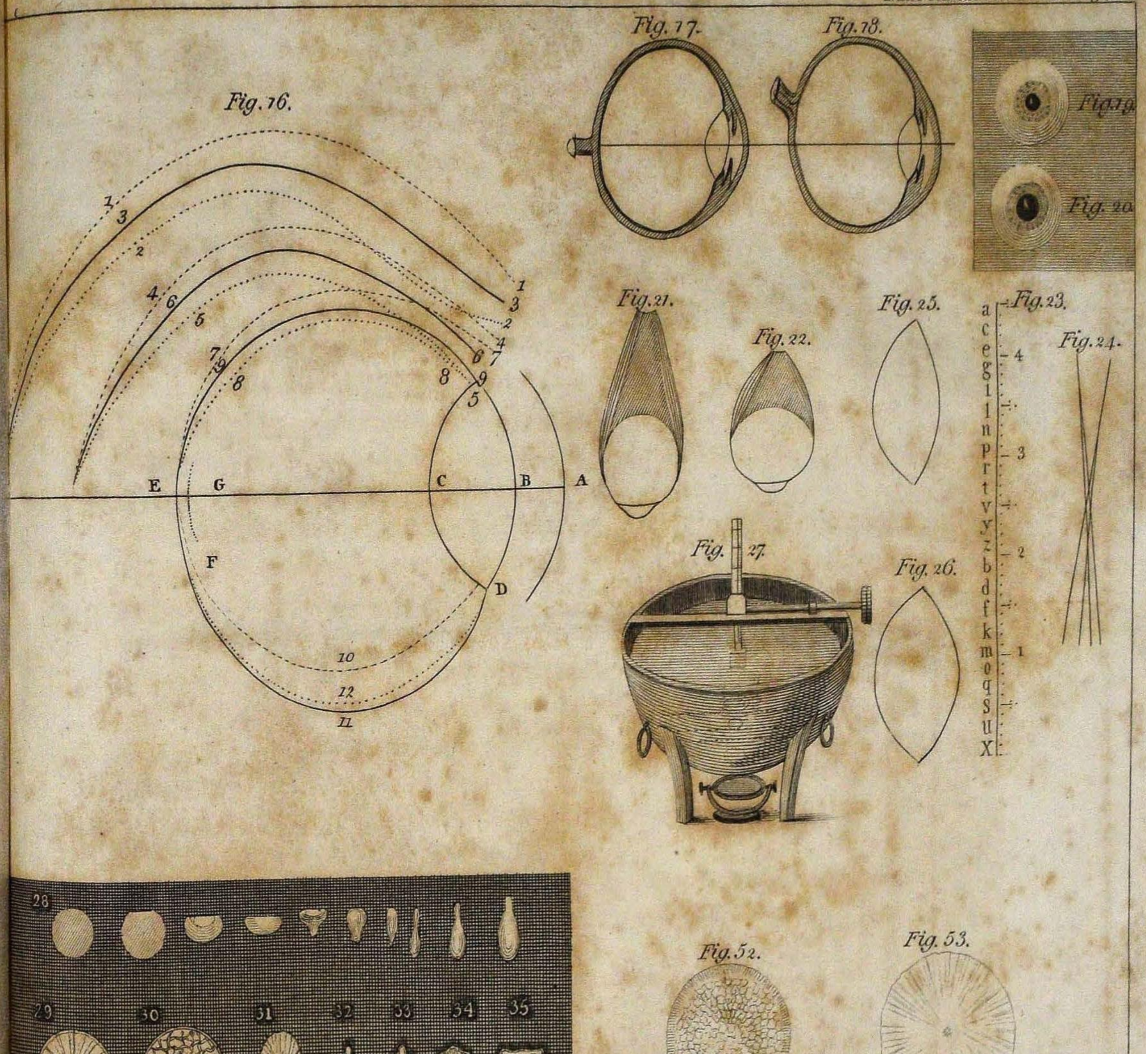
A man whofe mufcles of the left fide of the face, and the interior mufcles of the globe of the eye on the fame fide were paralyfed in confequence of a cold, had been electrified feveral times. He experienced no fenfation nor contraction when the affected part received the fpark, and there was fcarcely the flighteft contraction in the zygomaticus major when the electric fhock was applied. This man was fubjected to the galvanic action of a pile of fifty fetts, by forming a communication between the different points of the difeafed fide of the face, and the two extremities of the pile by means of metallic excitators.

At the moment of contact all the mufcles of the face became contracted. The patient felt pain with a very difagreeable fenfation of heat, the eye became convulfed, tears flowed involuntarily, and rednefs with fwelling was perceived on the different parts which were touched.

Thefe experiments, which appear to afford fome means of comparing the effects of galvanifm with electricity, were repeated feveral fucceffive days at the fchool of medicine, and on the 26th of Prarial Citizen Halle gave an account of the fame to the National Inftitute. He obferved, that the mufcles remained contracted for fome minutes after the galvanic commotion, and that the left eye even followed the motion of the right.

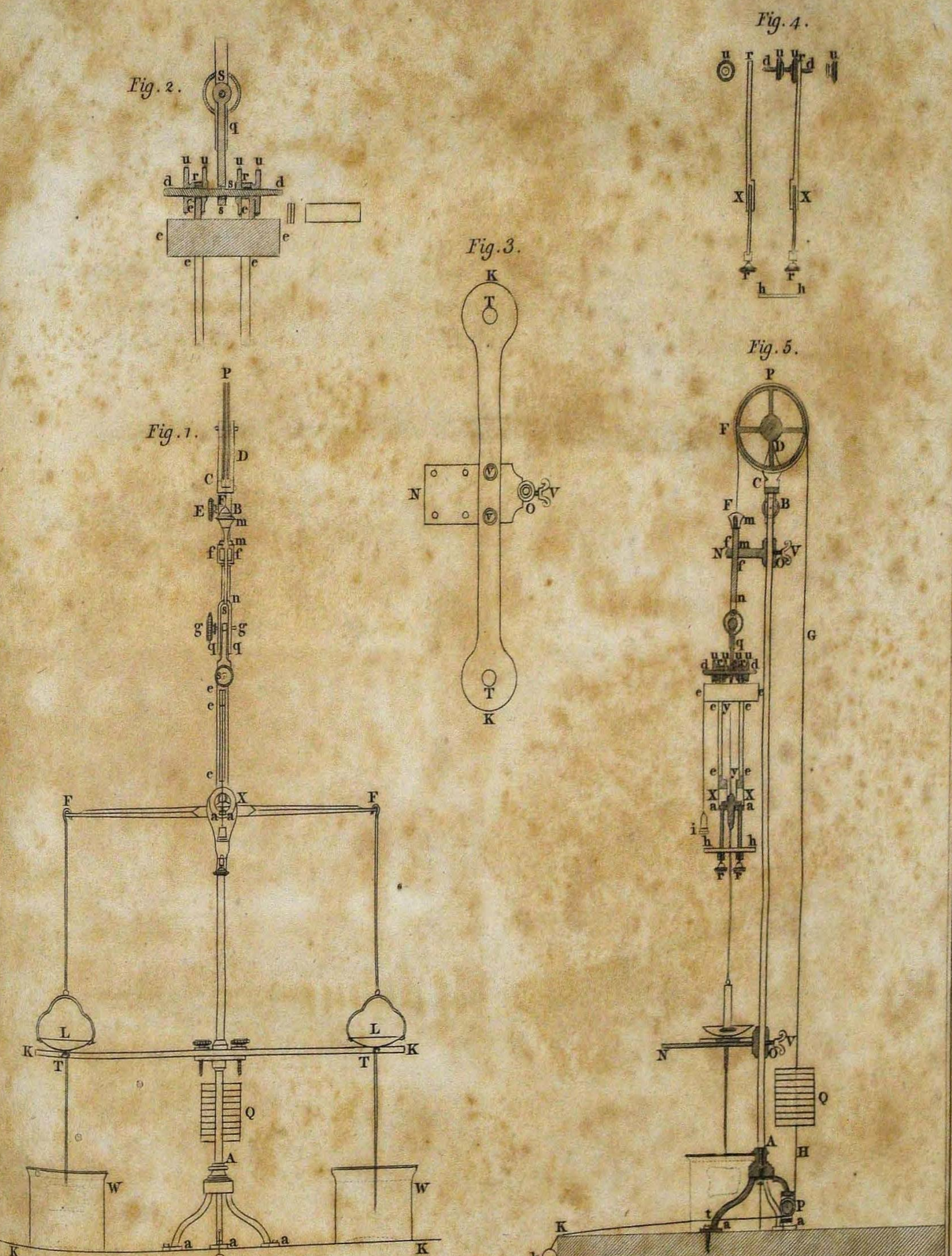
In this application of galvanifm to the human body, Citizen Halle obferved fome very fingular anomalies. The pile was often very long before it communicated its effect ; fometimes it ceafed to produce any effect for feveral feconds together. In both thefe cafes it feemed as if the fluid met fome obftacle in its courfe. For in thefe circumftances it was neceffary to moiften the fkin, to rub it, and to change the relative pofition of the rings, in order to make the communication. In general it was obferved, that in order that the fenfation fhould be fpeedy, it was not enough that the fkin fhould be wetted, but that it muft be, as it were, foaked and foftened with the water. He himfelf, as well as feveral other perfons, made trial of the fenfation which the galvanifm produces. It refembles in fome refpects that of the action of feveral needles forced at the fame time into the fkin. It is a pungent pain, accompanied with a fenfe of heat and a flight metallic tafte, when the exciters were applied near the falivary glands.

* Communicated to the National Inftitute of France, and inferted in the Bulletin des Sciences, No. 51, An. IX.



Mutlow Sculp.

Prony's universal Support for a Balance.



Mutlow Sc. Russell Court.